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ABSTRACT

REPORTED IS A STUDY TO DETERMINE THE RELATIVE EFFECTIVENESS OF A MEANINGFUL CONCRETE AND A MEANINGFUL SYMBOLIC MODEL IN LEARNING A SELECTED MATHEMATICS PRINCIPLE. SUBJECTS WERE FROM A SECOND GRADE POPULATION AND THEY WERE ASSIGNED TO THREE TREATMENTS. STUDENTS ASSIGNED TO TREATMENT 1 RECEIVED INSTRUCTION IN THE PRINCIPLE WITH A MEANINGFUL SYMBOLIC MODEL AND STUDENTS ASSIGNED TO TREATMENT 2 RECEIVED INSTRUCTION IN THE PRINCIPLE WITH A MEANINGFUL CONCRETE MODEL. STUDENTS ASSIGNED TO TREATMENT 3 DID NOT PARTICIPATE IN THE INSTRUCTIONAL PORTION OF THE STUDY. AT THE END OF THE INSTRUCTIONAL PERIOD, LEARNING WAS EVALUATED BY FOUR TESTS THAT MEASURED THE DEPENDENT VARIABLES. THE RESULTS INDICATED THAT CHILDREN IN THIS STUDY WERE NOT ABLE TO GENERALIZE THEIR LEARNING TO DEMONSTRATING A LEARNED PRINCIPLE ON AN UNFAMILIAR CONCRETE DEVICE. MEANINGFUL LEARNING ALONE DOES NOT ENSURE THAT THE APPLICATION OF A LEARNED MATHEMATICAL PRINCIPLE CAN BE RECOGNIZED. (RP)

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Technical Report No. 101

A STUDY OF THE RELATIVE EFFECTIVENESS OF A MEANINGFUL CONCRETE AND A MEANINGFUL SYMBOLIC MODEL IN LEARNING A SELECTED MATHEMATICAL PRINCIPLE

Report from the Project on
Analysis of Mathematics Instruction

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Center for Cognitive Learning
The University of Wisconsin
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The Wisconsin Research and Development Center for Cognitive Learning focuses on contributing to a better understanding of cognitive learning by children and youth and to the improvement of related educational practices. The strategy for research and development is comprehensive. It includes basic research to generate new knowledge about the conditions and processes of learning and about the processes of instruction, and the subsequent development of research-based instructional materials, many of which are designed for use by teachers and others for use by students. These materials are tested and refined in school settings. Throughout these operations behavioral scientists, curriculum experts, academic scholars, and school people interact, insuring that the results of Center activities are based soundly on knowledge of subject matter and cognitive learning and that they are applied to the improvement of educational practice.

This Technical Report is from Phase 2 of the Project on Prototypic Instructional Systems in Elementary Mathematics in Program 2. General objectives of the Program are to establish rationale and strategy for developing instructional systems, to identify sequences of concepts and cognitive skills, to develop assessment procedures for those concepts and skills, to identify or develop instructional materials associated with the concepts and cognitive skills, and to generate new knowledge about instructional procedures. Contributing to the Program objectives, the Mathematics Project, Phase 1, is developing and testing a televised course in arithmetic for Grades 1-6 which provides not only a complete program of instruction for the pupils but also inservice training for teachers. Phase 2 has a long-term goal of providing an individually guided instructional program in elementary mathematics. Preliminary activities include identifying instructional objectives, student activities, teacher activities materials, and assessment procedures for integration into a total mathematics curriculum. The third phase focuses on the development of a computer system for managing individually guided instruction in mathematics and on a later extension of the system's applicability.

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ABSTRACT

The purpose of the study was to determine the relative effectiveness of a meaningful concrete and a meaningful symbolic model in learning a selected mathematical principle. Subjects in a second grade population who measured at or above criterion level on a qualifying examination were assigned randomly to groups which were then assigned randomly to one of three treatments. Groups assigned to Treatments I and II received instruction in the same mathematical principle from the same teacher for the same length of time. The instructional periods were similar for groups receiving both treatments with the exception of the model used to make the mathematical principle meaningful. Groups assigned to Treatment I received instruction in the principle with a meaningful symbolic model and groups assigned to Treatment II received instruction in the principle with a meaningful concrete model. Groups assigned to Treatment III did not participate in the instructional portion of the study but served as control groups for one dependent variable measure.

At the end of the instructional period learning was evaluated by tests that measured the dependent variables. The Recall Test included problems to be solved stated in symbols used during instruction.

The Symbolic Transfer Test I included problems to be solved which were untaught symbolic instances of the principle. Subjests were permitted to use as aids in solving the problems the model with which they had learned. The Symbolic Transfer Test II included problems to be solved which were untaught symbolic instances of the principle. Subjects were permitted to use familiar concrete aids to solve these problems. The Concrete Transfer Test measured the ability to demonstrate the principle on an unfamiliar concrete device. The experimental and control groups' performances were measured on this test.

The data from these tests were analyzed collectively by a multivariate analysis of variance and individually by one-way analyses of variance. Inspection of the data and the analyses indicates: (1) The groups that learned with the symbolic model did somewhat better, although not significantly so in overall learning of the principle. (2) The groups which had learned with the symbolic model performed somewhat better but not significantly so on the test of direct recall. (3) The groups which had learned with the symbolic model performed better on the two tests of symbolic transfer. (4) There were no significant differences in performance on the test of concrete transfer between groups which had learned with the symbolic model, concrete model, or had received no instruction in the principle.

This study indicates that there were no significant differences in the overall learning of a mathematical principle when learning

was facilitated by a meaningful concrete or a meaningful symbolic model. Second grade children were able to learn a mathematical principle by using only a symbolic or a concrete model when that model was related to knowledge the children had. This provides evidence that making the teaching of mathematical principles meaningful is as important as are the materials used to demonstrate that principle.

While there were no significant differences in the overall learning of the principle, children who had learned with a symbolic model could transfer this learning to solving untaught symbolic instances of the principle significantly better than could children who had learned with a concrete model. Learning facilitated by a symbolic model was more easily generalized than learning facilitated by a concrete model. This suggests that meaningful symbolic teaching may be a more powerful instructional technique than has been recognized.

Children in this study were not able to generalize their learning to demonstrating a learned principle on an unfamiliar concrete device. Meaningful learning alone does not ensure that the application of a learned mathematical principle can be recognized.

CHAPTER I

THE PROBLEM

The objectives of mathematics instruction in the elementary school include the teaching of mathematical principles and their interrelatedness. This combination is often called the content of the mathematics program. What this content should be has been agreed upon fairly well as attested to by Begle (1966, p. 4): "We can agree on the broad outline of the content of the mathematics curriculum for the schools." Although there is this agreement upon what should be taught in the elementary school mathematics program, there is little agreement upon how learning environments can best be structured to facilitate the learning of this content.

One reason for the lack of agreement of how to structure mathematics learning environments is the lack of empirical evidence about the role played by various models in children's learning of mathematical principles. This study is designed to gather empirical data concerning the use of different models of mathematical principles in the learning environment of the elementary school.

I. Meaningful Learning

Agreement can be found that mathematical principles can best be learned in environments that include provision for meaningful learning. Such agreement has come about in part due to the body of research and theory concerning meaningful learning. Dawson and Ruddell (1955a, p.393) state that the years between 1938 and 1953 "have seen the gradual accumulation of a body of evidence or data, and/or descriptions of practice to support the meaning theory."

There is some consistency among mathematics educators as to how meaningful learning can be defined. Meaningful learning concerns the learner grasping the principles and their interrelationships which together make up the structure of mathematics, or the content of the elementary school mathematics program.

Dawson and Ruddell (1955a, pp. 393-394) say: "The mathematical aim has to do with meaning . . . the relationships which bind arithmetic into a system of thinking. . . . When arithmetic is taught according to the mathematical aim, learning becomes meaningful." Thiele (1941, p. 45) states that meaningful arithmetic is that "which seeks to help children to appreciate and utilize the interrelationships in the number system." Brownell (1947, p. 48) says "Meaning is to be sought in the structure, the organization, and the inner relationship of the subject itself." In a difference of semantics rather than belief, Van Engen (1953, p. 75) defines understanding in the way Brownell, Thiele, and Dawson and Ruddell define meaning when he says: "The pupil who understands is in possession of the cause and effect relationships, the logical implications and the sequence of thought that unite two or more statements by means of the bonds of logic."

Ausubel (1967, p. 19) accepts such a definition as being an integral part of meaningful learning, that part which he calls logical meaning. Logical meaning refers to "whether the material is relatable, on a non-arbitrary and substantive basis, to relevant ideas in any appropriately mature hypothetical cognitive structure."

Ausubel (1967, p. 19) adds another dimension to meaningful learning, psychological meaning, which is unique to each individual. "When an individual meaningfully learns logically meaningful concepts and propositions, then, he does not assimilate their logical meanings, but the invariably idiosyncratic psychological meaning that such learning induces in his particular cognitive structure." Van Engen (1953, p. 75) also speaks of this phase of meaningful learning which he says is the relationship that an individual sees between a referent and a symbol for that referent. "Meaning is that which is 'read into' a symbol by the pupil. The pupil realizes that the symbol is a substitute for an object."

While it is possible to talk about two phases of meaning, the two are closely interrelated and it is not within the scope of this study to separate them. This study is basically concerned with meaning in the logical sense and it is assumed that manifestation by an individual of such logical meaning is an indication that his own psychological meaning is equivalent to the logical meaning.

Research studies support the efficacy of the meaningful presentation of mathematical principles. A study reported by Brownell, Moser et al (1949) is often quoted in support of teaching meaningfully. This study investigated, among other things, two procedures for the teaching of subtraction (decomposition and equal additions) each of which was taught in different ways (meaningfully and mechanically). There were about 1400 third grade children enrolled in forty-one classrooms who served as subjects for the study. Learning was evaluated by assessing subjects' understanding of subtraction, speed and accuracy of computation, and transfer of learning to solving problems of untaught instances of subtraction.

Critical ratios were computed between scores on the various tests received by groups of subjects who had learned meaningfully and groups of subjects who had learned mechanically. In 102 instances the critical ratio found was significant in favor of the subjects who had learned meaningfully and in 37 instances a significant difference was found in favor of those who had learned mechanically. In 123 instances the critical ratio found was not significant. Brownell and Moser (p. 155) summarized the findings as showing: "The outstanding success in teaching decomposition rationally*; the greater difficulty of teaching equal additions rationally; and the much greater transferability of skill in borrowing when taught by decomposition rationally."

They also state that their evidence gives support to the idea that the effects of learning in a meaningful manner are cumulative as evidenced by "the relatively higher accuracy scores earned by R* sections in the Retention Test and in the Transfer Test" (p. 155).

Another study which sheds light on the role of meaningful learning is one reported by Thiele (1938) in which first graders received instruction in basic addition facts through one of two methods: a rote drill method or a meaningful method in which subjects were aided in seeing the relationships between various addition facts. At the end of the instructional period, learning was evaluated by tests which measured speed and accuracy of recall of the addition facts. The results were significant in favor of those groups which learned by the meaningful method.

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* The groups who learned meaningfully were called the rational (R) groups.

McConnell (1934) reports a study which was concerned with the learning of addition and subtraction facts by first graders. Half of the subjects learned in a rote manner which involved basically drill work on the various combinations while the other half learned each fact in relationship to other facts. The children in the latter group also discovered the combinations by manipulation of objects and when errors were made, children had to correct them through use of concrete objects. Learning was evaluated on the basis of accuracy, speed, ability to detect errors, ability to learn new skills independently and maturity in manipulating number facts. Significant differences were found in favor of the subjects which learned by rote on tests of accuracy and speed and in favor of the subjects which learned meaningfully on maturity of manipulating facts. All other differences found were not significant but favored the subjects who had learned meaningfully.

Swenson (1949) studied the learning of addition facts by second grade pupils. These children learned in one of three ways: discovery and generalization of the relationships between the facts at all times; drill only; and drill-plus where the children discovered the combinations and then drilled on them. Retention scores and net achievement gain did not differ significantly among children in the three treatment groups. However, pupils who had learned by the generalization and discovery method scored significantly higher on all tests of transferability.

Anderson (1949) reports a study of fourth grade children who received instruction by a method of teaching based on either a connectionist theory (drill) or a field theory (meaning) of learning. No significant differences were found in arithmetic achievement between subjects who were taught by either method but, a significant difference in favor of the meaning method was found in a test requiring transfer of learning. When subjects were separated according to general ability and initial arithmetic achievement, those who scored lower on the test of general ability but high in arithmetic achievement learned better by the drill method while those who scored higher on the test of general ability but low on initial arithmetic achievement learned better by the meaningful method.

Howard (1950) reported a study done with classes in grades five and six involved in learning addition of fractions. One group of children were told how to do the problems and then spent the majority of their time practicing computation. Another group used manipulative aids and charts which showed the interrelationships of the various ideas. This latter group solved many verbal problems utilizing the basic idea as their only practice in computation. The third group used the same materials as the second but also practiced computation as well as solving verbal problems. After the instructional period, a test was given in solving verbal problems and in computation. At the end of three months, the same test was given again. No significant differences were found at the end of the instructional period in either problem solving or computation but at the end of three months, a large significant difference was found in computation in favor of the group who had learned meaningfully and had practiced computation.

Pace (1961) reports a study in which she attempted to discover if making a problem solving operation meaningful, improved the problem solving ability of fourth grade children. Children in the experimental group received instruction in solving problems with the emphasis on how a problem was to be solved and why a certain operation was appropriate. The children in the control group solved the same problems, but they received no help in understanding why a certain operation was appropriate. At the end of the instructional period the control group had made only negligible gains while the experimental groups had made significant gains in problem solving ability and in arithmetic reasoning ability.

Krich (1964) reports a study in which sixth grade classes were matched on intelligence and arithmetic ability. Half the classes were taught meaningfully by having a rational explanation of what was involved in solving certain problems. The subjects in classes in the other half were given rules for solving problems. Each group worked with programmed materials. Learning was evaluated by a test designed to measure understanding and computational ability. At the end of the instructional period, no significant differences were found. However, on a retention test given later, the subjects who had learned meaningfully did significantly better.

In a study with a different emphasis, Shipp (1958) investigated the effects of different amounts of time devoted to activities calculated to develop meaning. In one group at grade levels four, five, and six, 75% of the time was spent on developmental-meaningful activities while the remainder of the time was spent on drill activities. In the other three groups at each grade level, 60%, 40%, or 25% of the time was spent on meaningful activities with the remainder of the time in each case being spent in drill activities. At the end of the instructional period, an achievement test was given. The groups that had spent 75% or 60% of their time on developing meaning had a significantly higher total score and scored significantly higher on understanding and computation.

In a similar study Shuster and Pigge (1965) investigated the retention of material by fifth graders when differing amounts of time were spent on developing meaning. Each treatment group spent 75%, 50%, or 25% of their time developing meanings in arithmetic and the remainder of their time on drill activities. At the end of the study no significant differences in learning were found but on a delayed recall test, the mean scores for computation and understanding of processes were significantly better in those groups who spent 50% or 75% of their time on developing meanings.

Miller (1957) assessed the effect of an educational program that emphasized meaning as opposed to one that emphasized memory. Seventh grade teachers were selected by a panel as typifying these approaches. The learning of their students was evaluated by a standardized test of arithmetic and an author constructed meaning test at the end of a semester's work and again at the end of summer vacation. Significant differences in favor of the meaning groups were found at the end of the summer on the standardized test and on the portion of the meaning test which measured the highest degree of arithmetic ability.

Another study done by Fullerton (1955) compared two methods of teaching multiplication of whole numbers. Half the classes involved were assigned randomly to a traditional method (use of a traditional textbook) and the other half were assigned to a meaning method which involved using counters, drawing pictures, number lines and story problems. Each class was taught by its own teacher. Recall and transfer tests were given at the end of the instructional period and three and a half weeks later. A significant difference was found in favor of those groups which had learned with the meaning method.

The quoted studies offer some evidence that children are able to comprehend mathematical ideas better when they see the interrelationships of various ideas. Although there seems to be enough evidence to indicate that meaningful learning is better than non-meaningful learning, there is little evidence to indicate which component of a learning environment facilitates such learning? The above studies offer little information on this point in part because they varied a great deal in their definition of what composes a meaningful learning environment. The studies were not designed within a mathematics curriculum research framework and did not consider all the important components of the mathematics curriculum either as independent or controlled variables.

II. Mathematics Curriculum Research

DeVault (1966) indentified four components of the mathematics curriculum: instructional activities, teachers, learners, and curriculum materials. Each of these components needs to be considered in some way "if research is to have a significant impact on the quality of education in the schools" (p. 639). In the studies just quoted, one or more of the components was uncontrolled in some way. The effect of teacher behavior was largely ignored in the Thiele, Swenson, Howard, Pace and Fullerton studies. The effect of materials was not accounted for in the Brownell and Moser, Thiele, Pace, and Anderson study. The McConnell, Fullerton and Miller studies used different instructional activities. The effect of the learner variable was controlled in most of the studies by matching individuals or groups usually on intelligence and/or arithmetic achievement, but Campbell and Stanley (1968, p. 15) state: "Matching is no real help when used to overcome initial group differences."

Due to the lack of controls on the important components of the curriculum, there is little information available as to what makes a learning environment meaningful. Therefore, in order to gain more specific information about what is involved in making a learning environment meaningful, this study was designed to control two components of the mathematics curriculum (teacher, learners), partially control and partially vary the other two components (instructional methods and materials) in an attempt to see what effect the use of different instructional materials and the related instructional activities have upon meaningful learning.

Mathematics curriculum materials are those things used in instruction. They include, among other things, text books, work books, and concrete or symbolic models used by the teacher or pupils in the teaching/learning of mathematical principles. One specific subset of this set of curriculum materials was selected to be the independent variable of this study, the concrete or symbolic model used in instruction and learning. That portion of instructional activities specifically related to the use of the selected instructional materials was also varied. All other instructional activities and materials were controlled.

III. Definitions

A. Mathematical Principle

A principle as defined for this study is one of many interrelated ideas which together make up the structure of a body of knowledge. What these principles are and how they are defined can be agreed upon by scholars. Such principles exist independently of and externally to individuals.

Holton (1952, p. 271-272) agrees that there are many interrelated principles in a body of knowledge when he lists three main elements which make up a physical science: "Concepts or constructs, relations between the concepts, and the grammar for expressing these." (Careful reading of some writers reveal that what they refer to as concepts is synonymous with the definition of a principle.)

A part of King and Brownell's (1966, p. 81) definition of an academic discipline is that it must have a conceptual structure and that "the conceptual structure of any discipline is the full set of ideas in a discipline at any one time."

In talking specifically about the discipline of mathematics, Allendoerfer, (1965, p. 8) concurs that mathematics is a structure which is an "abstract system of undefined words, axioms . . . built by mathematicians." Therefore since these principles are built by men, they can be identified, at least at an elementary level, and defined. Fehr (1966_a, p. 225-233) attempting to clarify for non-mathematicians, some mathematical ideas says: "A mathematical concept . . . is not a simple thing but a very complex, entity." He believes that some concepts such as number, are so complex, that they are "attained only by very few persons after many years of mathematical study." However, at least the beginning of these concepts can be identified by scholars as being a part of the "well defined structures of mathematics."

Begle (1966, p. 5) also believes that principles which can be defined make up the structure of mathematics. In describing the mathematics curriculum of the sixties he says: "The new curriculum differs radically from the old in that it includes the structure of the common mathematical systems, the basic mathematical concepts and their interrelationships as well as the basic mathematics facts and techniques." Bruner (1963, p. 7, p. 31) says that the structure of a subject becomes more understandable as the relationship between the principles is learned. He says, "To learn structure, in short, is to learn how things are related." "The curriculum of a subject should be determined by the most fundamental underlying principles that give structure to that subject."

B. Symbolic Model

A symbolic model of a mathematical principle is the representation of that principle in commonly accepted numerals and signs which denote mathematical operations or relationships.

C. Concrete Model

A concrete model of a mathematical principle is the representation of that principle in concrete three dimensional objects. It usually has movable parts which can be manipulated to demonstrate certain mathematical operations. From such a model, it is possible to abstract a mathematical principle. Concrete models only represent mathematical principles. They are not the principle. Allendoerfer (1965) describes the nature of mathematics as being an abstraction which represents or models a portion of nature or the environment of the child. A concrete model attempts to represent that nature or environment in such a way so that the child is able to abstract from it the mathematical principle that one wishes them to learn.

Others have defined concrete models in similar manner. Dienes (1963, p. 67) talks about physical embodiments of mathematical principles as "situations which are physically equivalent to the concepts." Williams (1961, p. 112) talks of concrete analogues which "represent in concrete form the element of arithmetical operations, and can be used as miming devices with which to parallel or 'mime' derivative arithmetical operations."

IV. Concrete Models and the Learning of Mathematical Principles

There is much theoretical evidence that young children learn mathematical principles better when learning is facilitated by concrete models which demonstrate those principles. Major support for this is provided by Piaget and his followers. Piaget is a psychologist who is concerned with identifying a comprehensive theory of cognitive development that will encompass individual growth from birth to maturity. According to this theory, as an individual grows, he forms mental schemas (mental structures) by a continual process of accommodation to and assimilation of his environment. This adaptation (accommodation and assimilation) is possible because of the actions performed by the individual upon his environment. "Actions performed by the subject constitute the substance or raw material of all intellectual and perceptual adaptation" (Flavell, 1963, p. 82). Without physical actions or physical interaction with the environment it is impossible for individuals to learn. The actions necessary for learning change in character and progress from overt, sensory actions done almost completely outside the individual; to partially internalized actions which can be done with symbols representing actions done before; to complete abstract thought. However, the later is "nothing but interiorized actions, whose efferent impulses do not develop into external movements" Piaget, 1954, p. 141).

Mathematics educators concur in believing that this type of learning environment facilitates learning. Van Engen (1953, p. 91) states that "the meaning of words cannot be thrown back on the meaning of other words." Meanings or understanding of mathematical principles must come from the child working with objects. "When the child has seen the action and performed the act himself, he is ready for the symbol for the act." Mathematical ideas are not innate. "If this is granted, then it becomes evident that the child must acquire mathematical ideas from events with the physical world." After he has acquired these ideas, then he should learn the symbolism for them.

Lovell (1966, p. 13) believes that mathematical principles are learned from "actual acquaintance with objects and situations, and through undergoing experiences and engaging in actions of various kinds." Only as the environment permits and encourages children to participate in such experiences with objects that model the mathematical principle they are to learn, will they be able to learn that principle.

Dienes (1963) supports the point of view that mathematical principles are not frequently encountered in the unstructured environment of the child. He feels that children are not surrounded by experiences that encourage abstraction and generalization of the important principles of mathematics. Only a few minimal principles of cardinal numbers, addition, subtraction and division would be formed as a child grows. Therefore, the environment must be structured so that the child meets mathematical principles in a concrete way.

Curriculum research which considers the use of concrete models in learning mathematical principles is meager and inconclusive. Price (1950, p. 7) stated: "Up to the present time few if any attempts have been made to determine the value of instruction which makes extensive use of multi-sensory aids in the teaching of a single process in arithmetic." The situation is not much different today.

Hamilton (1966, p. 465) states: The literature on the use of manipulative devices "contains little in the way of hard evidence. Much of the argument for their use is inspirational, based on theoretical considerations."

Further evidence of the lack of empirical curriculum research is found in a monograph which reports research proposals of the participants in a conference on Psychological Problems and Research Methods in Mathematics Training held at Washington University in 1959. These proposals for suggested research studies were designed to shed light on unanswered problems in the teaching and learning of mathematics. Vangerplas (1960, p. 123) suggested a research design which would explore the use of sensory-perceptual aids in learning mathematical principles. In this he argues persuasively that certain mathematical principles can be learned better by concrete materials while others can be learned better by symbolic means. However, he says there is no empirical evidence to give us knowledge of which type of aid is effective for which principle. To answer this question he proposed a study in which a single principle should be taught to groups of learners with various concrete or symbolic aids, and the resulting learning evaluated to determine which aid was more effective. Other investigators have also pointed to the lack of clear evidence for the effectiveness of sensory aids: Dodes (1953); Gibb (1956); Fehr (1947); Kinsella (1950); Lewis (1956).

More evidence that the problem has not been solved is seen in the first issue of the Journal of Research and Development in Education which was devoted to reporting the proceedings of the National Conference on Needed Research in Mathematics Education (1967, p. 45). The first of the list of research problems suggested as important by the participants was: "To what extent does experience with concrete materials improve learning in mathematics."

The largest group of studies which are available concerns one particular set of concrete models, the Cuisenaire rods and their prescribed teaching method (Gattegno, 1964). These rods consist of wooden blocks, graduated in size from a 1 cm cube to a 1 cm x 1 cm x 10 cm rod. Each size rod is a certain color. Basically learners are to play freely with the rods to discover specific numerical relationships; and then to learn the common mathematical symbols and use them as they manipulate the rods to discover more relationships. There have been a number of studies which have investigated the materials used in the prescribed way. These studies are basically the same. Two groups are equated in some way: one group is taught in a traditional way with traditional materials (often defined no better than this); the other group is taught with the Cuisenaire matherials as prescribed; and learning by the two groups is evaluated in some way. Table 1 is a summary of studies concerned with Cuisenaire Materials.

Aurich (1963), Hollis (1964), and Crowder (1965) worked with first graders. The total scope of the mathematics program appropriate for the grade level was the mathematics content, and learning was evaluated by a standardized test. In all the first grade studies significant differences

TABLE 1

SUMMARY OF STUDIES WHICH COMPARED CUISENAIRE MATERIALS AND TRADITIONAL MATERIALS

Author	Grade Level	Measurement Instrument	Significant Difference in Favor of:	Mathematical Content
Aurich (1963)	First	SRA Achievement Test	Cuisenaire Treatment	Total Range of First Grade Work
Hollis (1964)	First	Standardized Achievement Test	Cuisenaire Treatment	Total Range of First Grade Work
Crowder (1965)	First	Standardized Achievement Test	Cuisenaire Treatment	Total Range of First Grade Work
Nasca (1966)	Second	Standardized Achievement Test	Neither Treatment	Total Range of Second Grade Work
		Cuisenaire Achievement Test	Cuisenaire Treatment	
Passy (1963)	Third	Standardized Achievement Test	Traditional Treatment	Computation and Arithmetic Reasoning
Haynes (1963)	Third	Author Constructed Test	Neither Treatment	Multiplication
		Standardized Achievement Test	Neither Treatment	
Lucow (1963)	Third	Author Constructed Test	Neither Treatment	Multiplication and Division

in performance on the standardized tests were found in favor of the children who had learned with the Cuisenaire materials. Nasca (1966) worked with second graders and evaluated learning with a standardized test and a Cuisenaire test. Significant differences in performance on the Cuisenaire test were found in favor of those who had learned with the Cuisenaire materials. Non-significant differences were found when learning was measured by the standardized test.

Passy (1963), Haynes (1963), and Lucow (1963) worked with third graders. Passy was concerned with the learning of arithmetical reasoning and computational skills which he evaluated with a standardized achievement test. A significant difference was found in favor of those who had learned with traditional materials. Haynes was concerned with the learning of multiplication and Lucow with the learning of multiplication and division. Haynes evaluated learning with a standardized achievement test and an author constructed test while Lucow used only an author constructed test. Non-significant differences were found.

Several things are apparent from the results obtained in these studies. The Cuisenaire materials were more effective at the first grade level than were traditional materials. With older children the results are ambiguous. In one case the traditional method was more effective. In another case the Cuisenaire method was more effective when learning was measured by a test which included more advanced principles than were in the standardized test. In four cases no significant differences were found. Three explanations might account for such results:

Perhaps, the tests used for evaluation did not adequately measure what was learned. In most cases general arithmetic achievement was the content measured. This is done in standardized tests by symbolic means involving knowledge of and ability to compute with symbols. Ordinarily such tests do not measure the application of mathematical principles in solving of problems with concrete materials and as such probably are not true measures of what children learn as they work with concrete models.

Assuming Piaget's stages of development are accurate, children in the first grade are early in the operational stage where concrete interaction with the environment is necessary before symbolization of that interaction has meaning. Older children have less need for concrete interaction because previous concrete experiences have built their schema and they are ready to use symbols. Therefore, extra concrete interaction such as is provided by the Cuisenaire materials is superfluous.

The studies were inadequate in design and control, so the results are non-significant. None of these studies was set in the context of a theoretical position regarding the nature of mathematics curriculum. Thus, inadequate attention was given important variables either as independent, controlled or as dependent variables. The nature of these studies underscores the need for carefully controlled experimental studies in this area if the role of concrete materials in mathematics instruction is to be clarified.

Other studies have been done which utilize other concrete models in learning mathematical principles. In these studies learning by a traditional method of teaching (mostly undefined) is compared with learning facilitated by use of concrete models to supplement the traditional method.

Lucas (1966) reports a study with first graders in which the learning of children who had worked with the Dienes Attribute Blocks was compared to the learning of children who had learned in the way prescribed by the Greater Cleveland Mathematics Program. Learning was evaluated by a standardized arithmetic test and an author made test which measured degree of conservation of number and conceptualization of certain mathematical principles. The group who had learned utilizing the Dienes Attribute Blocks did significantly better on the conservation of number test and on the conceptualization of mathematical principles. The groups which had learned with the Greater Cleveland Mathematics Program materials did significantly better on computation and the solving of verbal problems.

Ekman (1966) was concerned with teaching third grade children addition and subtraction algorithms. One group was presented only with the algorithms: another group first worked with pictures which showed the principles and then worked with the algorithms: a third group worked with counters and then worked with the algorithms. At the end of the instructional period, learning was evaluated by the amount of growth on an author made test which measured understanding, computation and transfer of learning. No significant differences were found. However, on a retention test given later, a significant difference was found in understanding in favor of those who had used counters.

Dawson and Ruddell (1955b) compared learning by fourth graders who were taught division of whole numbers either by a traditional approach or by solving socially significant division problems using many concrete objects. An author-made test was administered at the end of the instructional period and seven weeks after instruction had ended. The groups which had learned with the concrete objects did significantly better on the test administered immediately following instruction, on the retention test and on a transfer test.

In another study of the learning of division with whole numbers, Norman (1955) compared learning facilitated by a traditional, textbook approach; learning when the division problems were put in socially significant setting; and learning facilitated by the use of concrete and semi-concrete models (number lines, drawings, and counters). There were no significant differences found in learning immediately following instruction, but two weeks later a significant difference was found in favor of those who had learned with concrete and semi-concrete models.

A study done by Howard (1950) was also concerned with the use of concrete models with children in grades five and six. No significant difference was found in favor of learning with concrete models or traditional methods at the end of the instructional period but a significant difference was found at the end of three months in favor of the group that had used concrete models.

Swick (1959), Mott (1959), Spross (1962), and Price (1950) worked with fifth and sixth grade subjects; Jamison (1962) worked with seventh grade subjects; and Anderson (1959) worked with eighth grade subjects. All compared learning facilitated by concrete models with learning not facilitated by concrete models. Learning was evaluated by author-made tests or by standardized tests. In none of these studies were significant differences in learning, retention, or transfer found.

Once again the same trend is evident that was seen in the studies done with the Cuisenaire materials. Younger children appear to learn more when learning is aided with concrete models of mathematical principles. The results of using such models by older children manifest themselves in a neutral way. They neither improve learning nor do they hamper it. Once again, three explanations can be made:

Piaget's stages are relatively accurate and older children can use symbols effectively when they represent actions the children have experienced.

The tests do not measure what has been learned.

The studies are inadequate in design and controls for the same reasons the Cuisenaire studies were inadequate.

Another explanation might be made which explains the results of the studies done with younger children. Could it be that the variable under consideration in the above studies was not the value of concrete or symbolic models in the teaching of mathematical principles, but the presence or absence of meaningful learning. In the treatments where concrete models were used, the principle to be learned was taught as part of the structure of mathematics. This would make the learning meaningful. In the treatments where symbolic models were used, there was little attempt to relate the principles in a "non-arbitrary and substantive basis" (Ausabel 1967, p. 19)to relevant mathematical ideas. This would make the learning non-meaningful.

A closer look at the studies reported by Ekman, Dawson and Ruddell, Norman, and Howard support the idea. In Ekman's study algorithms were presented to one group of learners in a rote non-meaningful way, while the algorithms were presented meaningfully to another group with meaning being derived from the manipulation of counters. In the Dawson and Ruddell study not only were concrete models used to give meaning to the division algorithm but a definition of division (subtraction of equivalent sets) was used which is based on knowledge the child had previously and is an integral part of the structure of mathematics. In the Norman study, aids such as number lines and counters were used. In order to use these the learners had to have learned some mathematical principles to which the new principle of division could be related. In the Howard study the author even talks of the emphasis upon 'why' certain computations are done and he used the concrete models to demonstrate this reason.

In these studies the significant variable appears to be not one of concrete or symbolic models but one of meaning or non-meaning. Children who were taught with concrete models were also taught meaningfully. The children who were taught with symbolic models were not taught meaningfully. One of the important variables affecting learning, i.e. meaning, appears not to have been considered in the studies dealing with concrete materials. These studies leave an important question unanswered. Can meaning be derived only from concrete models in the lower grades as Piaget would have us believe? Can it also be derived from symbolic models, if the symbolic models are related in a "non-arbitrary and substantive basis to relevant ideas in any appropriately mature hypothetical cognitive structure" (Ausubel 1967, p. 19)? Can the use of meaningful symbolic models facilitate learning equally as well as meaningful concrete models with young children? This study was designed to investigate this question.

V. The Major Problem and Hypothesis

The major problem of this study was to determine the relative effectiveness of a meaningful concrete and a meaningful symbolic model in facilitating learning of a specific mathematical principle. If the learning of children who use the meaningful symbolic or concrete model is not different, then the conclusion can be drawn that either model is an equally effective aid in the learning of mathematical principles.

To gain empirical evidence that would permit acceptance or rejection of such a statement, a study was designed to answer the following:

> Are there differences in the learning of a selected mathematical principle by groups of children who have learned that principle utilizing either a meaningful symbolic or a meaningful concrete model to give meaning to that principle?

> The major hypothesis (I) of the study stated in null form is:

> There are no significant differences in the learning of a mathematical principle between groups of children who have learned that principle using a meaningful concrete or a meaningful symbolic model.

VI. Secondary Questions and Hypotheses

Learning a mathematical principle can be evidenced in at least two ways: (1) the ability to recognize and solve instances of that principle as it was learned originally or specific recall of instances of the principle: (2) the ability to recognize and solve unlearned instances of that principle or the ability to transfer what has been learned. In order to gain information relevant to the major question under investigation, several subquestions related to these two dimensions of learning need to be investigated. The first question deals with specific recall and the second, third, and fourth are concerned with the transfer of training.

> Are there differences in the ability to recall instances of a mathematical principle between groups of children who have learned that principle using a meaningful symbolic model or a meaningful concrete model?
>
> Are there differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who have learned that principle using a meaningful symbolic model or a meaningful concrete model when each has the model with which they learned to use as an aid in problem solving?
>
> Are there differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who learned with a meaningful symbolic model or a meaningful concrete model when they have familiar concrete materials to use as aids?
>
> Are there differences in the ability to demonstrate the principle on an unfamiliar concrete model between groups of children who have learned the principle utilizing a meaningful symbolic model or a meaningful concrete model?

A. Specific Recall Questions and Hypothesis

> Are there differences in the ability to recall instances of a mathematical principle between groups of children who have learned that principle using a meaningful concrete or a meaningful symbolic model?

The studies concerned with meaningful learning that have been discussed previously, do not consistently indicate that if a child has learned meaningfully he is better able to immediately recall mathematical principles than if he has not learned meaningfully. The studies by Howard, Anderson, Swenson, Krich, Shuster and Pigge, and Miller suggest that there is little difference in achievement between those children who had learned mechanically or meaningfully when the learning is evaluated soon after the end of the instructional period. McConnell's and Brownell and Moser's studies suggest that those who learned mechanically did better in tests measuring speed and accuracy than those who learned meaningfully. Thiele's study suggests that those who learned meaningfully recalled the addition facts better at the end of the instructional period.

Since the results are ambiguous concerning the effect of meaningful learning upon recall, it might be assumed that meaningful learning in and of itself is not an important factor in immediate recall. Therefore, a recall test of learning administered soon after the end of the instructional period will not give much evidence as to whether or not the model used enables a child to learn meaningfully. Use of either model in the learning experience should produce the same amount of recall. Therefore, Hypothesis II stated in null form is:

> There is no significant difference in the recall of instances of a selected mathematical principle between groups of children who have learned that principle using a meaningful concrete model or a meaningful symbolic model.

B. Transfer of Learning Questions and Hypotheses

> Are there differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who have learned that principle using a meaningful symbolic model or a meaningful concrete model?
>
> Are there differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who learned that principle using a meaningful concrete or a meaningful

> symbolic model when they have familiar concrete materials to use as aids?
>
> Are there differences in the ability to demonstrate a learned mathematical principle on an unfamiliar concrete model between groups of children who have learned that principle with a meaningful concrete or a meaningful symbolic model?

"Transfer of learning occurs when whatever is learned in one situation is used in another situation." (Klausmeier and Goodwin 1966, p. 463). Learning that is meaningful better facilitates transfer than learning that is not meaningful. The meaning studies by Brownell and Moser, Swenson, Anderson used a test of transfer as a criterion of learning. The results obtained showed significant differences in learning favoring those who had learned meaningfully in all the studies.

Several classic studies outside the area of mathematics education support the point of view that principles which have been learned as an interrelated body of knowledge can be used to solve problems which are new instances of the learned principles. Hendrickson and Schroeder (1941) in a replication of a study by Judd divided eighth grade boys into three groups. All boys were given the opportunity to practice hitting a submerged target until they could do it successfully. Group I subjects were told they could practice until they could hit the target. Group II subjects were given instruction on the principle of light refraction. Group III subjects were told specifically that changing the depth of water changed the amount of refraction and were given the same instruction as subjects in Group II. The subjects learned to hit the target submerged six inches and then learned to hit the target submerged two inches. The number of trials necessary for success in hitting the target submerged two inches was the measurement used to assess transfer. Although large individual differences were found, the authors conclude; "Knowledge of theory was found to facilitate transfer. The completeness of the theoretical information had a direct effect upon both learning and transfer" (p. 213).

Another study often quoted to give credence to the idea that meaningful learning facilitates transfer is the one reported by Hilgard, Irvine, and Whipple (1953). Two groups of high school students were taught card tricks. One group rotely memorized the solutions while the other group was taught the tricks meaningfully. On the next day the subjects were asked to repeat the tricks they had learned and then to solve two kinds of problems requiring transfer. On one transfer test which required simple transformation, the meaning group did slightly better than the memory group. However, on the test requiring more problem solving, the meaning group did significantly better.

Another study which supports Hilgard et al is one reported by Fargus and Schwartz (1957). Three groups of college students were asked to learn new symbols for the alphabet. One group was told the principle by which the new symbols were related to the traditional alphabet. Another group was told there was a principle and were required to describe what the principle was. The third group was asked to memorize the new symbols. All groups worked to a specified criterion level. One week later a test of transfer was given in which the groups were asked to translate a paragraph written in a slightly different set of symbols. Another problem paragraph was given to be translated using a completely different set of symbols. The two groups which had learned with the meaning method were much superior to the memorization group on both translations.

Overman (1931) specifically studied factors which effect transfer of learning in arithmetic. Children in the second grade were taught addition either meaningfully or non-meaningfully. He concluded at the end of fifteen days of instruction that those children who were taught meaningfully were able to solve untaught examples better than those who were taught by rote.

A study in which semi-concrete objects and abstract drawings were used in the initial learning is reported by Reynolds (1966). In this study six groups of college students were asked to study a map on which pictures were drawn. They were also asked to study a list of nonsense syllables. One group had the nonsense syllables written on the map close to a specified picture such as a truck or an airstrip. The other five groups studied the list and map in a variety of unrelated ways. The transfer test was made up of eight statements that related the map picture and the nonsense syllables, i. e., "Pum is a truck driver". The subjects were to learn these statements. The group that had learned originally by studying the map with appropriately located symbols did significantly better than any of the other groups showing that learning in which the interrelationships can be seen is transferred better than learning in which interrelationships are not seen.

These studies offer evidence that meaningful learning transfers better than learning which is not meaningful. Therefore, if both concrete and symbolic models are equally effective in facilitating meaningful learning there will be no difference in the transfer of learning between groups of subjects who have learned using one model or the other. Therefore, Hypothesis III stated in null form is:

> There are no significant differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who have learned that principle using a meaningful symbolic or a meaningful concrete model when they used as aids in problem solving that model with which they learned.

Hypothesis IV stated in null form is:

> There are no significant differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who have learned using a meaningful symbolic model or a meaningful concrete model when they have familiar concrete materials to use as aids.

Klausmeier and Davis (1969) state that transfer is related to the similarity seen between the original task of learning and the transfer task. There is little evidence concerning the transfer ability of children of the learning of a mathematical principle to diverse concrete applications. Is this transfer easier if learning has been facilitated with a concrete or a symbolic model? Does the learning with symbols increase the generalizability to concrete models or does learning with concrete models enable children to transfer to concrete models. Hypothesis V investigates this.

Hypotheses V stated in null form is:

> There are no significant differences in the ability to demonstrate a learned mathematical principle on an unfamiliar concrete model between groups of children who have learned the principle using a meaningful symbolic or a meaningful concrete model.

C. An Additional Hypothesis

Perhaps children cannot solve instances of a learned mathematical principle on an unfamiliar concrete model. If they cannot, then children who have not learned the principle will be able to demonstrate the principle on the unfamiliar concrete device equally well. Hypothesis VI investigates this and stated in null form is:

> There are no significant differences in the ability to demonstrate a mathematical principle on an unfamiliar concrete model between groups of children who have had instruction in the principle with a meaningful concrete or a meaningful symbolic model and groups of children who have received no instruction in that principle.

VII. Summary

Empirical evidence indicates that children learn mathematical content better when it is presented meaningfully. Piaget and his followers present theoretical evidence to support the belief that learning in young children is also better when it is facilitated by the use of concrete models. However, the empirical studies reported which have investigated the use of concrete models have failed to consider the important variable of meaningful or non-meaningful learning. Studies which support the belief that concrete models facilitate learning better than do symbolic models, have in reality usually made the learning facilitated by concrete models meaningful and the learning facilitated by symbolic models non-meaningful. This study was designed to investigate the learning of a selected mathematical principle by groups of children who learned that principle using either a meaningful concrete model or a meaningful symbolic model.

The study was designed to control two components of the mathematics curriculum (teacher and learners), and to partially control and partially vary the other two components (instructional materials and instructional activities). The independent variable of the study was one specific subset of the curriculum materials and the instructional activities specifically related to it: i.e. the concrete or symbolic model used to teach a specific mathematical principle. The dependent variables were measures of two dimensions of learning: specific recall and transfer of learning to solving problems of both symbolic and concrete representations of the principle. An additional question was investigated dealing with the ability to demonstrate a mathematical principle on an unfamiliar concrete device by groups of children who had learned that principle and groups of children who had not learned it.

CHAPTER II

METHODS AND PROCEDURES

In this chapter the theoretical and specific design of the study will be made explicit. The manner in which curriculum components are controlled and the manner in which they serve as independent variables will be discussed. The dependent variables and the analyses of the data will be described.

I. Theoretical Design of the Study

The four components or research variables of a mathematics curriculum (DeVault, 1966) are learners, teachers, curriculum materials, and instructional activities. Each of these components was specifically attended to in the design of the present study. The *learners* component was controlled by complete randomization of subjects. A single teacher taught all treatments thus providing the control for the *teacher* component. The *curriculum materials and instructional activities* components were partially controlled by having portions of them identical for the experimental treatments. The portions of both which were used to give meaning to the mathematical principle to be learned were varied. The independent variable for the study was the way in which meaning was given to the mathematical principle; i.e. through the use of a concrete or symbolic model. Learning was evaluated by a series of tests (dependent variables) designed to give information related to each of the hypotheses.

II. Specific Design of the Study

A population was tested on a qualifying examination. Those who met or exceeded criterion level on the qualifying examination were assigned randomly to groups and these groups assigned randomly to either an experimental treatment (Treatment I or Treatment II) or to control groups (Treatment III).

Subjects in groups assigned to Treatment I were taught the selected mathematical principle meaningfully through the use of a symbolic model. Subjects in groups receiving Treatment II were taught the selected mathematical principle meaningfully through the use of a concrete model. Subjects receiving Treatment III were not taught the mathematical principle but were used as control groups for one dependent variable.

An experimental teacher was selected. Each group in Treatment I and II attended experimental sessions for an equivalent length of time. At the end of that time, the effectiveness of the two treatments was evaluated by a series of tests designed to measure:

a. Ability of the subjects to recall instances of the mathematical principle as taught to all subjects.

b. Ability to solve problems which were untaught instances of the principle with the subjects using whatever models they had used during the experimental treatments.

c. Ability to solve problems which were untaught instances of the principle with the subjects using a concrete model that was not used in the experimental treatments but was familiar to both groups.

d. Ability to demonstrate instances of the principle on an unfamiliar concrete model. Subjects in Treatment III served as control groups for this dependent variable.

Data collected from these tests were analyzed collectively by a multivariate analysis of variance, and analyzed individually by a one-way analysis of variance.

The overall study design is shown in Table 2.

III. Controlled Variables

A. The Learners

A population was selected; their mathematical background previous to the study ascertained; and a qualifying examination administered. Those subjects who achieved at or above criterion level were assigned at random to one of three treatments.

1. Population

The total population of the Oregon, Wisconsin Elementary School second grade of 148 children were the subjects of this study.

Oregon, Wisconsin is a small community whose school serves a large rural area. There are also a number of people who live in the town who commute daily to Madison, Wisconsin. There is only one elementary school in the community. All second grade children had been randomly assigned by the principal of the school to six classrooms taught by six teachers.

During the preceding year the mathematics program used by the school was _Patterns in Arithmetic_*, _First Grade_. This program

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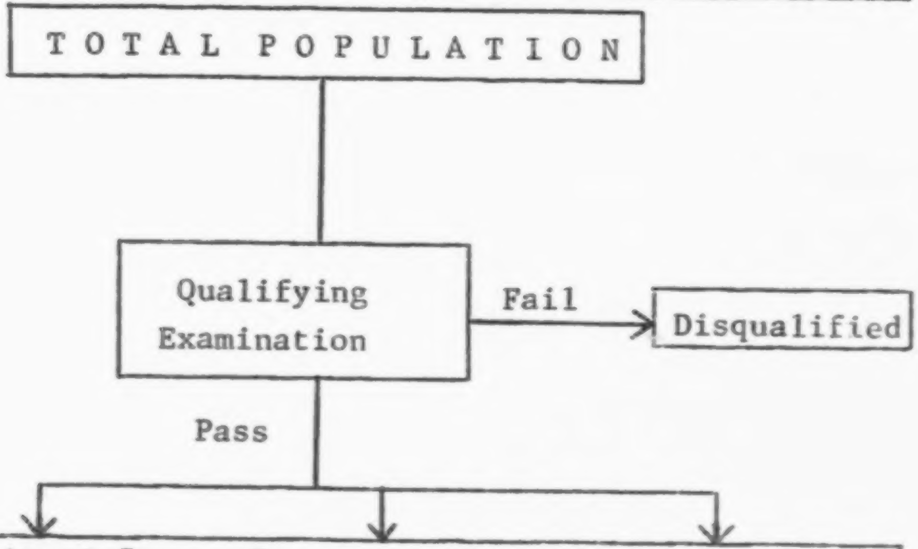
*A Telecourse in Arithmetic from the National Center for School and College Television, developed by Henry Van Engen at the Research and Development Center for Cognitive Learning, University of Wisconsin.

TABLE 2

DESIGN OF THE STUDY


TOTAL POPULATION
Qualifying Examination
Fail
Disqualified
Pass

	Treatment I Symbolic Model				Treatment II Concrete Model				Treatment III Control			
Groups	D	E	G	L	A	F	J	K	B	C	H	I
Experimental Treatment	x	x	x	x	x	x	x	x				
Recall Test	x	x	x	x	x	x	x	x				
Symbolic Transfer Test I	x	x	x	x	x	x	x	x				
Symbolic Transfer Test II	x	x	x	x	x	x	x	x				
Concrete Transfer Test	x	x	x	x	x	x	x	x	x	x	x	x

included lessons in all the prerequisite knowledge but did not contain specific instruction in multiplication, which was the mathematical principle selected to be learned during the instructional period of the study. Therefore, it was assumed that most of the children would not have had any formal training in multiplication. The mathematics program which was to be used in the classroom was the second grade edition of Patterns in Arithmetic. These televised programs did not start until one week before the study commenced, so no specific mathematics program was in use in the school during the pre-study period. Therefore, certain pages of a specific workbook were selected to be used in mathematics instruction until the televised programs and the study started. These pages reviewed the concept of number, and addition and subtraction concepts with one digit numbers.

The teachers were asked not to teach in any way the following topics: multiplication; division; counting by twos, threes, fours or fives; or even-odd numbers. They were also asked not to use Cuisenaire Rods or balance beams in any way as these were to be used in either an experimental treatment or in evaluation of learning.

To confirm that the teachers respected these requests, they were asked to keep informal diaries of the topics they taught before the study began.

Analysis of the diaries suggests that the teachers felt free to explore topics other than the ones specifically suggested but they also respected the request not to teach topics that would be directly related to mathematical principle which served as the content of the study. Table 3 shows the topics taught and number of teachers teaching each topic.

TABLE 3

TOPICS TAUGHT BY CLASSROOM TEACHERS PRECEDING THE STUDY

Topic	Number of Teachers Teaching Topic
Ordering of Numbers	4
Roman Numerals	1
Ordinal Numbers	3
Recognition of Equivalent and Non-equivalent Sets	4
Greater than and Less than	6
Addition and Subtraction up to 10	4
Grouping by 10's	3
Recognition of Numerals and Number Words	6
Counting to 100	2

2. The Qualifying Examination

One week before the study started, all children in all classrooms were given a qualifying examination, a copy of which can be found in Appendix A. This test was administered by a member of the project staff and was composed of items designed to measure the subjects' grasp of the knowledge required before learning the selected principle: cardinal number of a set, equivalent sets, numeral recognition (1-10), and addition of one digit numbers.

When the results of this test were analyzed it was found that only four children in the six classrooms missed any items on the test which measured learning of cardinal number of a set, equivalent sets, or numeral recognition, and these children missed only one item. However, the addition portion of the test showed more variation in response. The data for this portion of the test appear in Table 4. Fifty children missed no items, twenty children missed one item, twenty-seven children missed two items. Nine children missed nine or more items. Since knowledge of addition of one digit numbers was considered essential for learning the selected mathematical principle, subjects who missed more than nine of the items were eliminated. This arbitrary criterion level was selected because there was a natural break in the data at this point and it seemed reasonable to assume that those who missed as many as 45% of the test had failed to achieve an understanding of addition of one digit numbers.

TABLE 4

NUMBER OF ITEMS ANSWERED CORRECTLY ON QUALIFYING EXAMINATION, PART II, (ADDITION OF ONE DIGIT NUMBERS)

Number of Items Answered Correctly on Concept of Addition	Number of Subjects Receiving Score
20	50
19	20
18	27
17	14
16	10
15	3
14	7
13	4
12	1
11	2
10	3
9	2
4	1
0	1
Total	145*

* Data was missing on three subjects who were ill the day the test was administered. These three were included in the study on the recommendation of the classroom teacher that they were above grade level in achievement in mathematics and had the knowledge tested on the Qualifying Examination. It would have been impossible to test these subjects prior to the start of the study because of time limitations.

"The two major standards for ensuring content validity are (1) a representative collection of items and (2) "sensible' methods of test construction" (Nunnally 1967, p. 81). To meet these criteria for validity the total domain of content for addition of one digit numbers was randomly sampled and presented in a way that the subjects had met before.

To determine the reliability of the qualifying examination a Hoyt Reliability Coefficient (Hoyt, 1941) was computed and found to be .85.

3. The Treatment Groups

The children in each classroom who performed at or above criterion level on the Qualifying Examination were divided randomly into two groups. The resulting twelve groups (two from each of six classrooms) were assigned randomly to one of three treatments. The means and standard deviations of these treatment groups on the Qualifying Examination are reported in Table 5.

Due to different classroom sizes and to the elimination of different numbers of children from each classroom, the resulting experimental groups differed slightly in number. The number in each group for each treatment is reported in Table 6.

At least one group from each classroom participated in the experimental treatments. When two groups were used from one classroom, they were assigned to different treatments.

Treatment I was designated as the treatment for which the meaning of the principle would be derived from a symbolic model. Treatment II was designated as being the treatment where the subjects would derive the meaning of the principle from the use of a concrete

TABLE 5

MEANS AND STANDARD DEVIATIONS OF TREATMENT GROUPS ON QUALIFYING EXAMINATION (PART II)

				Treatment				
	I			II			III	
Group	Mean	SD	Group	Mean	SD	Group	Mean	SD
D	18.4	5.68	A	18.0	2.14	B	17.7	6.32
E	17.8	1.90	F	18.1	1.37	C	18.0	6.20
G	18.1	1.68	J	18.8	2.09	H	18.4	2.30
L	17.8	2.40	K	17.2	2.33	I	19.5	.78

TABLE 6

NUMBER OF SUBJECTS IN EACH GROUP AND ASSIGNMENT TO TREATMENT

Classroom	Group	Treatment I	Treatment II	Treatment III
1	A		11	
	B			10
2	C			10
	D	11		
3	E	12		
	F		11	
4	G	12		
	H			11
5	I			13
	J		13	
6	K		12	
	L	13		
Total Number		48	47	44

model. Treatment III received no instruction in the mathematical principle selected.

"The most adequate all-purpose assurance of lack of initial biases between groups is randomization" (Campbell and Stanley 1968, p. 25). The total population was assigned at random to classrooms; children within each classroom who met criterion on the qualifying test were assigned at random to a group; groups were assigned at random to an experimental treatment. It is assumed that the groups were equivalent in their knowledge of the principle at the beginning of the study.*

B. The Teacher

1. Selecting the Teacher

The teacher selected to teach the experimental groups was selected on the basis of: knowledge of mathematics, understanding of the use of the concrete model, understanding of the experimental procedure and independent variable, and teaching ability. Names of possible applicants who might meet the qualifications were obtained from professional educators. Three applicants were interviewed. Their knowledge of mathematics was determined by inspection of their formal background and by questioning. Their understanding of the concrete model was determined by asking them to demonstrate various mathematical principles with the model. An explanation of the independent variable of the study was given to the applicants and they were asked to prepare informal sample lesson plans utilizing the two models. The teaching ability of the applicants was determined in two ways. Former employers were asked to give recommendations. The applicants were then asked to teach a demonstration lesson to a group of children. These demonstration lessons were video taped and examined later to determine the relative teaching ability of the applicants.

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* To check the equivalence of the groups, a one-way analysis of variance was computed with intelligence test scores and qualifying examination scores. Means and standard deviations of the groups' intelligence test scores and the results of these analyses can be found in Appendix B. These analyses indicate no significant differences in the groups on the basis of intelligence and qualifying examination scores.

On the bases of highest level of performance on the criteria a teacher was selected who held a B.S. degree with a double major in Education and Science with an area of concentration in mathematics. She had previously taught two years and was highly recommended by her former principal and Director of Instruction. During the interview she consistently exhibited a high degree of understanding of the mathematics, the concrete model and the manipulation of the independent variable. During the demonstration lesson, she developed rapport quickly with the children and demonstrated her ability to control the independent variable as she was instructed to do.

2. Training the Teacher

After the teacher was selected, a short training period followed. She was asked to study the content background of the study (Fehr and Hill, 1966b, pp. 163-176); descriptive material about the concrete model (Gattegno 1964); and a complete description of the study.

The teacher was also asked to teach several lessons to the study director which utilized the independent variable. From these lessons, it was decided that the teacher understood the mathematical content and independent variable.

3. Teacher Behavior

The study director observed 21 of 112 of the classes taught by the experimental teacher for the purpose of determining the extent to which teacher behavior was consistent with plans for each treatment. These classes were selected at random and included at least one observation of each group. These observations were of instructional sessions of groups in one treatment or of instructional sessions of groups in different treatments but having the same instructional objective for the day.

Of particular interest was the teacher's treatment of the independent variable. To ascertain this, the observer tabulated on a form prepared for this purpose the number of times the teacher attempted to make the mathematical principle meaningful and whether this was done in a symbolic, semi-concrete, or concrete way. A copy of this Observation sheet can be found in Appendix C. Teacher behavior was noted when the teacher made a statement that related a model to the principle: i.e., two fives go with ten because five plus five equals ten; or the entire group's attention was focused on answering a specific question. Tabulations were not made if the teacher asked a question, the answer to which was to be determined by each subject in a manner prescribed by the treatment. In these instances, the teacher moved around the group and checked to see if each of the subjects had made the correct response. Often in such a situation, the teacher would explain the problem to be solved meaningfully to one individual but as it was often times out of hearing range of the observer, it was decided not to tabulate any such statements by the teacher.

Table 7 shows a summary of the groups observed and the percentage of meaningful statements in the various modes made by the experimental teacher.

From the observations it appeared that the experimental teacher was able to restrict her statements to giving meaning only in the way prescribed by the treatment. In none of the classes observed by the study director did she attempt to make the principle meaningful in a symbolic way for those groups in Treatment II or in a concrete way for those groups in Treatment I.

The teacher's method of instruction was basically the same for all groups. It involved much question asking. Answers given by the children were questioned as to their correctness whether they were correct or not. There appeared to be little problem with discipline in any of the groups. During the first week of the study subjects wore name tags so they could be identified by the teacher. However, by the second week the teacher knew the names so the need for tags was eliminated. The teacher worked with a quiet voice and reserved manner at all times. She was consistent in the standards she set for behavior and the subjects responded well to her.

On the basis of observations, the research component represented by the teacher appeared to be well controlled. The teacher was able to teach all groups in as like a manner as possible while varying the independent variable in the way dictated by the treatment.

An outside observer observed three groups (G, E and F) being taught on the twelfth day of the study. The instructional objectives for the day were drill and review so little was done in the way

TABLE 7

TEACHER OBSERVATIONS SUMMARY

Group Observed	Day of Study	Number of Tallies	% Symbolic	% Semi-Concrete	% Concrete
		Treatment I			
D	2	11	100	0	0
G	2	17	100	0	0
G	4	6	100	0	0
E	4	8	100	0	0
D	8	5	100	0	0
E	8	2	100	0	0
L	8	1	100	0	0
G	12	2	100	0	0
E	12	8	100	0	0
		Treatment II			
F*	2	0	0	0	
F	4	1	0	0	100
A	5	21	0	0	100
J	5	30	0	0	100
F	8	0	0	0	
F	9	0	0	0	
K	8	1	0	0	100
J	8	0	0	0	
J	9	0	0	0	
K	9	0	0	0	
K	10	0	0	0	100
F	10	1	0	0	

* Children were learning the new definition of number. The mathematical principle was introduced as symbolism.

of meaningful teaching. However, this observer agreed that the teacher was treating the groups similarly except in the case of how they were encouraged to solve problems. Groups D and E were groups receiving Treatment I and were treated alike in asking them to solve problems using only symbols. Group F was receiving Treatment II and was asked to solve problems using the concrete model.

C. Curriculum Materials

Curriculum materials are comprised of the content of an academic discipline and various things used to teach that content. The portion of the curriculum materials research variable which was controlled was all materials not specifically related to helping the subject attach meaning to the mathematical principle selected. These controlled materials included the mathematical content, its symbolic statement and materials used for symbolizing it.

1. Mathematical Content and Its Symbolization

The mathematical principle selected as the content to be learned was:

> With any given ordered pair of counting numbers, there can always be associated a unique counting number under the operation ultimately to be identified as multiplication.

This reflects the definition of Van Engen, et. al. (1965, p. 92) who say that the operation of multiplication for natural numbers is "a many to one mapping whereby every ordered pair of natural numbers is mapped onto a natural number that is their product." This principle was selected as the focus for the study because it is an important mathematical principle; it had not been taught to the subjects selected for the study; and it requires only mathematical knowledge contained in the first grade mathematics program of the subjects. In order to learn a principle, certain knowledge must be possessed prior to learning. Gagne (1962, p. 356) talks of a "hierarchy of subordinate knowledge" which it is necessary to possess before one can successfully perform a higher order task. The subordinate knowledge required for learning the selected principle as delimited for this study is: cardinal number of a set, equivalent sets, addition with one digit numbers, and recognition of numerals. That this knowledge is required for learning multiplication is recognized by many authors of textbook series when such knowledge is introduced prior to the operation of multiplication.

In the experimental sessions, the learning of instances of this principle was restricted to pairs of numbers whose products were equal to or less than ten. In the evaluation instruments, the principle was restricted to pairs of numbers whose products were equal to or less than 16.

The symbolic statement of the principle used in both groups was in the general form $a,b \rightarrow c$ where a,b represents an ordered pair of numbers which is associated with c. Arabic numerals were used to represent numbers. It is recognized that this symbolism is not necessarily unique to the operation of multiplication. It could stand for other operations. However, the usage is justified in this study because it was used only in this context. By using a statement not commonly associated with multiplication in the elementary school, undue influence outside the experimental sessions could be partly eliminated.

2. Other Materials

All groups used an easel, paper and pencil, and chalk and chalkboard in symbolizing the principle. All groups also did worksheets, copies of which can be found in Appendix D. The number and type of worksheets were identical.

D. Instructional Activities

An attempt was made to control all portions of the instructional activities except those which had to do with making the mathematical principle meaningful.

1. Organization of Instruction

An unused room in the Oregon Elementary School was designated as the experimental classroom. Each group of children receiving Treatment I and Treatment II came to the classroom at a specific time each day for fourteen consecutive school days for a twenty-five minute period of instruction. The time when each group came to the experimental classroom was determined by the school principal and was followed with the exception of two days when certain groups had to be rescheduled due to special events taking place in the school. This rescheduling was done so that each group met with the experimental teacher on each experimental day.

At the end of each experimental class period, the teacher wrote the following information on a form provided, a copy of which can be found in Appendix E: Purposes of Instruction, Activities, Materials Used, Length of Instructional Period, Interruptions, and Number Absent. These provided a summary of the experimental treatments.

Each group was scheduled for a 25-minute period daily.

This schedule and the mean and standard deviations of the actual time spent in the experimental classroom by each group is shown in Table 8. With only five exceptions the time each group spent in the experimental classroom was within three minutes of the twenty-five minute period that was planned. Group D and Group G each spent one fifteen minute session; Group L spent one twenty minute session, and Group J spent one thirty minute session in the experimental classroom. The time spent in the classroom was basically the same for all groups in all treatments.

Because of the location of the experimental classroom and the cooperation of the school personnel, interruptions to the experimental classroom sessions were held to a minimum. During one session of Group D (Session 9), a fire drill was held and the subjects were out of the classroom five minutes. During one of Group K's sessions (Session 1) the janitor came into the classroom and talked briefly with the experimental teacher. In one of Group L's instructional sessions (Session 7) the teacher found it necessary to talk with one subject in the hall briefly. These were the only interruptions that specifically interferred with the classroom procedure. Observers came in and went out at the back of the classroom during sessions of all groups. It appeared that subjects ignored such interruptions.

Table 9 shows a summary of the absences of subjects from the various experimental groups. Since only three subjects were absent from more than two experimental sessions, it was felt that the number of absences affected the learning of the experimental groups little.

TABLE 8

DAILY SCHEDULE FOR EXPERIMENTAL GROUPS

Group	Treatment*	Schedule Time	Minutes Actually Spent in Experimental Classrooms	
			Mean	SD
D	I	9:20-9:45 A.M.	24.9	3.11
G	I	9:50-10:15 A.M.	24.3	2.43
E	I	10:30-10:55 A.M.	25.1	1.55
F	II	11:00-11:25 A.M.	25.4	2.08
K	II	12:40-1:00 P.M.	25.2	.80
L	I	1:10-1:35 P.M.	24.6	.70
A	II	2:25-2:50 P.M.	25.3	.97
J	II	2:55-3:20 P.M.	25.0	1.78

* Treatment III Groups did not receive any instruction.

TABLE 9

ABSENCES FROM INSTRUCTIONAL SESSIONS

Group	Number of Children Absent	Number of Absences	Total Absences	Absences
		Treatment I		
D	2	1	2	1%
G	4 2	1 2	8	5%
E	2 1 1	2 3 4	11	7%
L	0		0	
Total	12		21	
		Treatment II		
F	1 1	1 4	5	3%
K	1 1	1 2	3	2%
A	2 1	1 2	4	3%
J	3	1	3	2%
Total	10		15	

2. Purposes of Instruction and Daily Activities

The purposes of instruction and activities to implement them were planned by the study director. They were the same for a day for those groups receiving the same treatment and after the first two days were approximately the same for all groups receiving both treatments. On the first instructional day and most of the second, the purpose of instruction for those groups receiving Treatment II was to familiarize the subjects with the concrete model selected. Subjects in Treatment II had never associated number with this model. Therefore, the purpose of instruction on the first two days for these groups was to develop an association of number with the Cuisenaire Rods. This association was not involved for those groups receiving Treatment I. However, in order to equalize the time spent in the experimental environment and to familiarize them with this environment, groups receiving Treatment I came to the classroom on the first day and did activities unrelated to the study. Instruction which led to the learning of the specific principle started at the beginning of the second instructional period for groups receiving Treatment I and near the end of the second instructional period for groups receiving Treatment II. These purposes of instruction and activities were determined by the order of introduction of the sets of numbers to be associated. It was decided to start with all pairs of numbers to be mapped on 10; then to procede to all pairs of numbers to be mapped on 9, 8, . . . 1. This order was considered appropriate because there are more pairs of numbers to be associated with the larger numbers and as such provided more exemplars for the subjects.

As soon as all possible sets of numbers had been introduced, drill and recall activities were selected by the study director in collaboration with the experimental teacher. Table 10 provides a summary of the purposes of instruction and the daily activities of groups in both treatments.

Groups in both treatments were required to solve the same problems by filling in blanks in partially completed sets of numbers. The blanks were left in all possible positions. For instance, all groups in both treatments were asked to solve the following problems on the ninth day of instruction.

2, 2 → ____
7, ____ → 7
3 → 1, ____
1, 1 → ____
5 → 1, ____
4, ____ → 8
____, 3 → 6

2, ____ → 10
3, ____ → 6
8 → 2, ____
3, ____ → 9
10 → ____, 2
4 → 1, ____
10 → 10, ____

Problems such as these were written on the chalkboard to be copied or appeared on worksheets to be done.

Drill activities for groups receiving both treatments were the same. Flash cards were used either with the total group responding, teams responding or individuals responding. These were 8 1/2 by 11" sheets of construction paper with problems written on them with magic markers, and they were used on the twelfth, thirteenth, and fourteenth days of instruction.

TABLE 10

SUMMARY OF DAILY PURPOSES OF INSTRUCTION AND ACTIVITIES

Instructional Day	Purpose of Instruction		Activity	
	Treatment I	Treatment II	Treatment I	Treatment II
First	To familiarize subjects with environment	To acquaint subjects with concrete model and to learn definition of number based on model	Pattern finding with ordered set of numbers Teacher read To Think I Way it on Mulberry Street. (Seuss, 1937)	Free play with the rods Measuring each rod with the white rod Building a staircase Practice with rod names
Second	Introduce Symbolism Find factors of 10	To acquaint subjects with concrete model and to learn definition of number based on model	Find factors of 10 Reading of Symbolism	Building a staircase Practice with rod names

TABLE 10 (continued)

Instructional Day	Purpose of Instruction		Activity	
	Treatment I	Treatment II	Treatment I	Treatment II
Third	Review factors of ten Find factors of nine	Review number definition	Find factors of ten Find factors of nine Worksheet No. I	Building a staircase and practice with rod names
Fourth	Review factors of 10, 9 Find factors of 8, 7	Review number definition Find factors of 8	Find factors of 10, 9, 8, 7	Building a staircase Find factors of 10, 9, 8 Worksheet No. III

TABLE 10 (continued)

<table>
<tr><th rowspan="2">Instructional Day</th><th colspan="2">Purpose of Instruction</th><th colspan="2">Activity</th></tr>
<tr><th>Treatment I</th><th>Treatment II</th><th>Treatment I</th><th>Treatment II</th></tr>
<tr><td>Fifth</td><td>Review factors of 10, 9</td><td>Review factors of 10, 9, 8
Find factors of 7</td><td>Find factors of 10, 9</td><td>Find factors of 10, 9, 8, 7</td></tr>
<tr><td rowspan="3">Sixth</td><td colspan="2">Review factors of 10, 9, 8</td><td rowspan="2">Find factors of 10, 9, 8, 7, 6, 5</td><td rowspan="2">Find factors of 10, 9, 8, 7, 6</td></tr>
<tr><td>Find factors of 7, 6, 5</td><td>Find factors of 7, 6</td></tr>
<tr><td colspan="2">Generalize findings</td><td colspan="2">Worksheet No. V</td></tr>
<tr><td rowspan="3">Seventh</td><td colspan="2">Review factors of 10, 9, 8</td><td rowspan="3">Find factors of 10, 9, 8, 5, 4
Worksheet VI</td><td rowspan="3">Find factors of 10, 9, 8, 7, 6, 5
Worksheet VIIa, VIIb</td></tr>
<tr><td colspan="2">Learn to solve problems</td></tr>
<tr><td>Find factors of 5, 4</td><td>Find factors of 7, 6, 5</td></tr>
</table>

TABLE 10 (continued)

Instructional Day	Purpose of Instruction		Activity	
	Treatment I	Treatment II	Treatment I	Treatment II
Eighth	Review factors of 5, 4 Find factors of 3, 2, 1 Learn to solve problems	Reveiw factors of 7, 6, 5 Find factors of 4, 3, 2, 1	Find factors of 5, 4, 3, 2, 1 Problem solving on chalkboard	Find factors of 7, 6, 5, 4, 3, 2, 1
Ninth Tenth	Review and Drill of all factors Problem Solving		Copy and fill in blanks of problems listed on board. Show solutions with: symbolic model	concrete model
Eleventh	Review and Drill of all Factors		Drill Games Flash Cards Worksheet No.IV Demonstrate answers with appropriate model	

TABLE 10 (continued)

Instructional Day	Purpose of Instruction		Activity	
	Treatment I	Treatment II	Treatment I	Treatment II
Twelfth	Review and Drill of all Factors		Drill Game Worksheets Nos. IX X, XI	
Thirteenth	Review and Drill of all Factors		Flash Cards Worksheets Nos. XII, XIV	
Fourteenth	Review and Drill of all Factors		Flash Cards Worksheet XV and XVI	

IV. The Independent Variable

The selected mathematical principle was to be learned meaningfully. How meaning was to be given to the principle is the independent variable of this study and determined the major area of difference in the two treatments. The independent variable was comprised of any portion of the instructional activities and curriculum materials that were related to making the mathematical principle meaningful.

A. Treatment I (The Symbolic Model)

The symbolic model selected to give meaning to the mathematical principle was the symbolic statement of the operation of addition of equal addends in the general form $\underbrace{b + b + b \; . \; . \; . + b}_{a \text{ addends}} = c$ which would correspond to the symbolic statement of an instance of the principle $a,b \rightarrow c$.

Whenever a set of numbers or symbols that exemplified the principle was given to subjects receiving this treatment, the reason for those particular numbers or symbols to be associated was verified in other symbolic terms involving this model either verbal or written. Whenever previously unlearned sets of number associations were to be found, subjects receiving this treatment found these correct associations by resorting to the symbolic model. For instance, in finding all the ordered pairs that go with six, the teacher might first write on the board $2 + 2 + 2 = \underline{\qquad}$. Then she would ask "How many twos do I have? Do three twos go with six?" The children, with the teacher's guidance, would then explore all the combinations of twos until they were convinced they had found all the correct sets that go with six. These would then be recorded on the chalkboard or the easel and the teacher would lead them through the same procedures with ones, threes, . . . sixes until they were convinced they had found all the sets that "go with six." The subjects participated in this type of activity directed by the teacher whenever they found sets that go together, or whenever they participated in review activities that were not specifically drill.

The subjects in groups receiving Treatment I were taught to solve the problem type exemplified by 3,2 → _____ by interpreting this to mean: $2 + 2 + 2 = 6$. If the problem had been 2,3 → _____, it would have been interpreted $3 + 3 = 6$. The problem type exemplified by _____, 3 → 6 was to be solved by finding how many threes were required to add to 6. 3, _____ → 6 was to be solved by using various numbers as addends 3 times until an answer of 6 was found. Any problems solved during the first eleven instructional days was accompanied by showing the solution in the symbolic model terms.

B. Treatment II (The Concrete Model)

The concrete model selected to give meaning to the mathematical principle was the Cuisenaire Rods (Gattegno, 1964). They define number based on length.

Subjects receiving Treatment II used this model extensively. On eleven of the fourteen instructional days, all children in Treatment II used the rods. They were available for use on the remaining instructional days and were used voluntarily by seven subjects in two different groups to solve problems.

Before children in this treatment could begin to learn the mathematical principle, they first had to learn to associate the concrete model with number.* To this end, subjects were taught to build a staircase of the rods. This was always done by this group and served as a reference for finding which number name to attach to which block. This staircase was built and rods given number names by measuring each rod with the one rod to see how many one rods could be put together to be the same length as the original rod. By the end of the third day the subjects used the staircase easily in naming the rods. Although they were encouraged to build it as a reference daily, many children seemed to know the number names of the rods by the end of the fifth day of instruction.

After subjects associated number with the concrete model, whenever a set of numbers or symbols were given, the reason for associating those particular numbers or symbols was proved by actual manipulation of the concrete model by the children. To find the numbers that "go with" nine, children would take the nine rod and attempt to fit sets of like rods together until they measured the same length as the nine rod. All such sets that measured the same length as the nine rod were recorded or symbolized in the same way as in Treatment I. 9,1   9, read as "nine ones go with nine," means that nine one rods placed end to end were the same length as one nine rod. Each set of factors was found for numbers 1 . . . 10. All other possible sets were tried by the subject under the direction of the teacher and rejected as being untrue.

Subjects in groups receiving Treatment II were taught to solve problems by using the rods. 3,2 → ____ was solved by referring to their staircase to find what color the two rod was, taking 3 two rods and placing them end to end, and finding which rod was as long as these three rods. Then by referring back to their staircase, they could find the number name of the answer rod. _____, 2 → 6 was solved by selecting a six rod and finding how many two rods would fit. Then by referring back to the staircase to determine what number name the rod had, the answer would be found. Any problems solved during the first eleven days of instruction were to be accompanied by showing of the solution with the rods.

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* The shortest or white rod was always referred to as the "one" rod.

On two worksheets an attempt was made to help the learners attach meaning to the mathematical principle. For groups receiving Treatment I, the appropriate symbolic model appeared next to a symbolic representation of the principle. For groups receiving Treatment II, space was left for children to place the correct concrete model beside the symbolic representation.

V. The Dependent Variables

The dependent variables were measured by a series of tests which directly reflect the hypotheses of the study.

Copies of all tests can be found in Appendix A. The purpose, scoring procedure, reliability, and validity of each test is summarized in Table 11.

A. Recall Test

The purpose of the Recall Test was to gather data to answer the second question of the study.

TABLE 11

TESTS OF DEPENDENT VARIABLES -- SUMMARY

Test	Scoring	Reliability	Validity
Recall Test	Number Correct out of 27 Items	Hoyt* r = .91	Content
Symbolic Transfer Test I	Number Correct out of 23 Items	Hoyt* r = .92	Content
Symbolic Transfer Test II	Number correct out of 23 Items	Hoyt* r = .93	Content
Concrete Transfer Test	Time Required to Complete Items Number of Trials Required to Complete Items	Anova** on Tester obtained scores (Inter Tester Reliability)	Content

** (Hays, 1966)
* (Hoyt, 1941)

> Are there differences in the recall of instances of a mathematical principle between groups of children who have learned that principle using a meaningful symbolic model or a meaningful concrete model?

The Recall Test was composed of problems in which instances of the mathematical principle were stated in the same way they were stated in the experimental instructional sessions. All sets of numbers that were used in the experimental sessions (products ≤ 10) were used in the test and arranged randomly. The missing part of the problem varied in location. The number of correct responses represented the test score. The reliability of this test as determined by the Hoyt (1941) reliability formula was $r = .91$.

The Recall Test was administered in the classroom regularly used for experimental instruction. The test was administered by a member of the staff on the day following completion of instruction. Groups were tested at the same time of the day they had been scheduled for the experimental sessions. Each subject was permitted as much time as he wished to complete the test. The test was taken by all subjects on the same day with the exception of one subject who was ill and took the test one week later.

B. Symbolic Transfer Test I

The purpose of the Symbolic Transfer Test I was to gather data to answer the third question of the study.

> Are there differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who have learned that principle using a meaningful symbolic model or a meaningful concrete model?

This test was composed of 23 problems (instances of the mathematical principle) which utilized all possible pairs of numbers whose products are 11 . . . 16. These problems were arranged randomly and the missing part of the problem varied in location. The number of correct responses represented the test score. The reliability of this test as determined by the Hoyt reliability formula was r = .92. This test was administered the day following the administration of the Recall Test A by the same tester. All groups came to the experimental classroom at the regularly scheduled time. Children were permitted all the time they wished. Subjects in Groups receiving Treatment I were provided with extra paper and told they could use it if they wished. Subjects in Groups receiving Treatment II were given sets of Cuisenaire Rods and told they could use them to solve the problems if they wished. No other help was given the subjects. All children completed the test by the end of 25 minutes.

C. Symbolic Transfer Test II

The purpose of the Symbolic Transfer Test II was to gather data to answer the fourth question of the study.

> Are there differences in the ability to solve problems of untaught instances of a mathematical principle of groups of children who learned with a meaningful symbolic model or a meaningful concrete model when they have familiar concrete materials to use as aids?

This test was identical in format to the Symbolic Transfer Test I. Each child was given in excess of 40 counters to serve as aids in solving the problems. The number of correct responses represented the test score. The reliability of this test as determined by the Hoyt reliability formula was r = .93.

The Symbolic Transfer Test II was administered one week after the Symbolic Transfer Test I. Each child was given the test and the counters and told to solve the problems using the counters. One half of each group was started on the test at approximately the same time. As each subject completed the test he returned to his classroom and sent another subject to be tested. Subjects were allowed as long as they wished to finish the test. All subjects in all groups were tested on the same day.

C. Validity - Recall and Symbolic Transfer Tests I and II

The purpose of these first three tests was to measure directly the ability of subjects to recall or to solve problems of a selected mathematical principle symbolized in a specific way.

In these three tests the total domain of content as specified in the hypotheses was included and arranged in the test randomly. Therefore, it must be concluded that Nunnally's (1967) two criteria were met and that the three tests had high content validity.

D. Concrete Transfer Test

The purpose of the Concrete Transfer Test was to gather data useful in answering the fifth and sixth questions of the study.

> Are there differences in the ability to demonstrate a selected mathematical principle on an unfamiliar concrete model between groups of children who have learned the principle using a meaningful symbolic model or a meaningful concrete model?
>
> Are there differences in the ability to demonstrate a selected mathematical principle on an unfamiliar concrete model between groups of children who have learned the principle using a meaningful concrete model, a meaningful symbolic model or have received no instruction in the principle?

The model selected for use in demonstrating the principle was a balance beam. Weights humg at appropriate numbered points balance the beam, e.g. three weights hung on point 2 on one side can be balanced by one weight on point 6 on the other side. Therefore, 3,2 → 6.

The test was divided into two parts. In the first part of the test the subject was handed one weight with which he was to balance a specific number of weights placed on one point on the beam by the tester. For example when the tester put four weights on point 2 on one side of the beam, the correct response for the subject was to place his one weight on point 8 on the other side of the beam. There were 13 items on this part of the test.

On the second part of the test, the tester would hang one weight on one side of the beam and hand the subject a specific number of weights to be hung on a single point on the other side. For example, the tester would hang one weight on 10 and hand the subject five weights: 10 → 5, _____. There were twelve items on this part of the test. Part I of the test was always done first.

Items were selected for this test partly as a result of a pilot study done before the actual testing of the experimental subjects. Including all possible combinations in both parts of the test made the test too long and the subjects lost interest or became tired. The items in which one was a factor proved to be non-discriminatory as all pilot study subjects did them in only one trial after the first one or two examples. Due to this, items in which one appears as a factor were randomly assigned to either Part I or Part II of the test. These items came first in both parts as they had appeared easier to pilot test subjects and it was hoped they would give each subject a measure of success as well as help them learn there was a specific response that would be correct. All other possible pairs of numbers whose product was equal to or less than 10 were included and arranged randomly on both parts of the test.

During the test the child was permitted as many trials as necessary in order to balance the beam. The tester recorded the number of trials necessary to balance and then proceeded immediately to the next item. At the end of Part I, the watch was stopped and the time recorded. Directions for Part II were given; the watch started, and the same procedures followed for Part II. Six scores are available for Test D: Time, Part I; Time, Part II; Total Time; Trials, Part I; Trials, Part II; and Total Trials.

1. Training the Testers

Five advanced graduate students in mathematics education and the study director were trained and used as testers for the Concrete Transfer Test. During training a demonstration of the test was given with second grade children who were not in the study. The testers were then given an opportunity to give the test to other subjects of the same age who were not in the study. The testers were watched and their procedure discussed. They continued to give the test to non-study subjects until it was felt the testing procedure was understood by the tester.

Complete written instructions were also given to each tester. A copy of these can be found with the test in Appendix A.

This test was administered individually to each subject by one of the six testers. On the fifth, sixth, and seventh school day after the end of the experimental study, the experimental groups receiving Treatments I and II were tested. Each tester was assigned subjects to test. They would test 2 or 3 subjects from one experimental group and then test 2 or 3 subjects from another group. At no time did the testers know in which experimental treatment the subjects had participated.

Subjects in Treatment III groups were also given this test to determine if either Treatment I or Treatment II had any effect upon the ability of subjects to demonstrate the principle upon the selected model. All subjects in groups in Treatment III were tested by the same tester during the final week of the experimental study.

The number of subjects in each group tested by the various testers is shown in Table 12. This type of assignment of subjects to testers was done to equalize the effects of a single tester across groups and across treatments.

Each subject came to a room where he and the tester were alone. The tester and subject were seated side by side with the balance beam on a table in front of them. The tester first made sure the subject understood balancing and that the beam could be balanced by placing one weight on one peg or by placing several weights on a single peg. During this pretest procedure the back of the beam was turned to the subjects and the points were not numbered. The pretest procedure was not timed and the subject was permitted to see instances of balancing until the tester felt he understood balancing. The beam was then turned so the subject could see the numbers. The tester had the subject point out

TABLE 12

NUMBER OF SUBJECTS EXAMINED ON CONCRETE TRANSFER TEST BY EACH TESTER

Group	Treatment	Tester	Number of Subjects Examined
D	I	1	3
		2	5
		3	2
		6	1
E	I	1	6
		2	3
		5	3
G	I	1	3
		2	2
		3	3
		4	3
		6	1
L	I	1	3
		2	4
		3	2
		4	3
		5	1
A	II	1	3
		2	5
		5	3
F*	II	1	5
		2	3
		5	2
J	II	1	5
		2	3
		4	4
		5	1
K	II	1	3
		2	3
		3	3
		4	3
B**	III	6	9
C	III	6	10
H	III	6	11
I	III	6	13

* During the test, the stop watch broke while testing one subject, so data is incomplete for this group.

** Data incomplete for one subject.

both sets of numbers and in which direction they got larger. The tester then started the stop watch and proceeded through the test in the order specified on the record sheet.

2. Reliability and Validity

The Concrete Transfer Test was designed to measure the subjects' ability to demonstrate the selected mathematical principle. When the beam balanced, the principle was demonstrated. The domain of possible items (or combinations of sets of numbers) was completely tested and these items were arranged randomly. Therefore, the content validity of this test is high.

The intertester reliability is high. The testers were to count the number of trials and to time with a stop watch how long it took to complete the parts of the test. To determine if there were differences due to tester variation among the six testers, several one way analyses of variance were computed (Hays, 1966). Analyses were made using the scores obtained by each tester on Time, Part I; Time, Part II; Total Time; Trials, Part I; Trials, Part II; and Total Trials. However, because the results of the Anovas computed on the parts of the test were in general agreement with the results of the Anovas computed on the total scores, only the analyses done on the Total scores are reported here. The Anovas of the scores on Time, Parts I and II and Trials, Parts I and II are reported in Appendix F.

The Anovas computed on the total time required for subjects to successfully complete the test when tested by the various testers resulted in an F ratio of .99 which with 5 and 128 degrees of freedom gives in a p of less than .42. There were no significant differences between the various testers on the amount of time required for the subjects to complete the test. The results of this analysis are shown in Table 13.

The Anova computed on the total number of trials required for subjects to successfully complete the test when tested by the various testers resulted in an F ratio of .24 which, with 5 and 128 degrees of freedom, gives a p of less than .94. There were no significant differences between the various testers on the number of trials required for the subjects to complete the test. The results of this analysis are shown in Table 14.

Therefore, it is concluded that because the F ratios found had probabilities which were so large, there were no significant differences between the testers on the results they obtained on the Concrete Transfer Test. The intertester reliability of this test was high.

VI. Statistical Analyses of the Data

A. Analyses

The computations for all analyses were done at the University of Wisconsin Computing Center.

The data collected from all measures of dependent variables were analyzed collectively by a multivariate analysis of variance (Manova)* using group means as a basis of analysis.

The data collected from each test were analyzed separately by one-way analysis of variance for fixed effects using group means as a basis for analysis. The alpha level chosen for significance was .05.

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*Jeremy D. Finn's "Multivariance: Fortran Program for Univariate and Multivariate Analysis of Variance and Covariance" was used for the multivariate and univariate analyses. The program came from the School of Education, State University of New York at Buffalo.

TABLE 13

ANALYSIS OF VARIANCE OF SCORES BY TESTERS ON CONCRETE TRANSFER TEST: TOTAL TIME

Source of Variation	Sum of Squares	df	Mean Square	F Ratio	P
Between	137583.9	5	27516.8	.99	.42
Within	3539101.4	128	27649.2		

TABLE 14

ANALYSIS OF VARIANCE ON SCORES BY TESTERS ON CONCRETE TRANSFER TEST: TOTAL NUMBER OF TRIALS

Source of Variance	Sum of Squares	df	Mean Square	F Ratio	P
Between	541.7	5	108.3	.24	.94
Within	56933.1	128	444.8		

Due to the ease of computation, analyses of variance were also computed using individual scores as a basis of analysis. Although such analyses do not meet a basic assumption underlying analysis of variance (Hays 1966, p.364), that of independence of observations, the results generally agree with what was found when using means as a basis of analysis. These results are reported in Appendix G.

B. Assumptions Underlying Use of Anova and Manova Techniques

The assumptions which underlie use of Manova and Anova techniques are similar (Braswell and Romberg, 1969). One assumption is that the measures of the dependent variables are arrived at independently. This assumption was met in this study by using the mean scores of the instructional groups as the basis of analysis. The instructional interaction was within groups and the between groups interaction was kept to a minimum.

Another assumption is that the variances and distributions of each variable are normal and in the case of Manova, the distribution of all scores is multivariate normal. Although this assumption was not tested, it seems reasonable that normality was approximated. The design of the study included complete randomization of subjects, which is some guarantee of normality. As the data were inspected, there was nothing to suggest marked deviation from normality. Both Manova and Anova are robust tests and even when the assumption of normality is not met the results should be reasonably valid.

Another assumption is that the model on which the study is built has to take into consideration all the variables that might effect the measures of the dependent variables. In educational research, it can never be assumed that all variables have been considered. However, in the present study, the model used for mathematics curriculum research seems a reasonable attempt to account for known important variables.

VII. Summary

This study was designed within a mathematics curriculum framework which is composed of four components: learners, teachers, curriculum materials, and instructional activities. Each of these components was specifically attended to in the design of the present study. The learners component was controlled by complete randomization of subjects. A single teacher taught all treatments thus providing the control for the teacher component. The curriculum materials and instructional activities components were identical for experimental treatments except for the portions of both which were used to give meaning to the mathematical principle to be learned. The independent variable for the study was the way in which meaning was given to the mathematical principle: i.e. through the use of a concrete or symbolic model. Learning was evaluated by a series of tests (dependent variables) designed to give information related to each of the hypotheses.

The second grade population of the Oregon, Wisconsin Elementary School was given a qualifying examination. Those who achieved at or above criterion level were divided randomly into groups and groups assigned randomly to one of three treatments. A teacher was selected and trained to teach all experimental treatments. A mathematical principle was selected to be the focus of instruction. Groups assigned to an experimental treatment received instruction for fourteen days from the teacher, for approximately the same length of time, using materials that were the same except for those which were used to give meaning to the principle. Groups assigned to Treatment I received instruction with a meaningful symbolic model. Groups assigned to Treatment II received instruction with a meaningful concrete model. Groups assigned to Treatment III received no instruction but were used as control groups for the Concrete Transfer Test.

At the end of the instructional period learning was evaluated by the four tests designed to measure the dependent variables: Recall, Symbolic Transfer Test I, Symbolic Transfer Test II, and Concrete Transfer Test.

Data from each test were analyzed by a one-way analysis of variance. Data collected from all tests were analyzed collectively by a multivariate analysis of variance.

CHAPTER III

RESULTS

In Chapter III the data collected from the measures of the dependent variables and their analyses will be presented. These dependent variable measures directly reflect the hypotheses of the study and were measured by the four tests: the Recall Test, Symbolic Transfer Test I, Symbolic Transfer Test II, and the Concrete Transfer Test. The first set of data and analyses is concerned with the results achieved on the four tests by the groups which had participated in the experimental treatments. The other set of data and analysis deals with results achieved on the Concrete Transfer Test by the control and experimental groups.

I. Data for Measures of Dependent Variables (Experimental Groups)

A. The Recall Test

The purpose of the Recall Test was to measure the ability of the subjects to recall the selected mathematical principle which had been the focus of instruction during the experimental treatments. The test was composed of 27 items and the score received was the number of items answered correctly. The distribution of scores, means and standard deviations of the experimental groups on the Recall Test are reported in Table 15.

The mean scores of groups in Treatment I ranged from 24.7 to

TABLE 15

DISTRIBUTION OF SCORES, MEANS AND STANDARD DEVIATIONS FOR EXPERIMENTAL GROUPS ON THE RECALL TEST

	Treatment									
	I					II				
	Instructional Group					Instructional Group				
Score	D	E	G	L	Total	A	F	J	K	Total
27	6	5	7	6	24	6	5	3	6	20
26	2	1	3	3	9	1	0	1	3	5
25	0	1	2	4	7	1	1	2	0	4
24	0	2	0	0	2	1	0	0	1	2
23	0	2	0	0	2	1	1	1	1	4
22	1	0	0	0	1	0	2	2	0	4
21	1	0	0	0	1	1	1	0	0	2
20	1	0	0	0	1	0	0	1	0	1
19						0	1	0	0	1
18						0	0	1	0	1
17	0	1	0	0	1	0	0	1	0	1
.										
.										
.										
6						0	0	1	0	1
.										
.										
.										
0						0	0	0	1	1
N	11	12	12	13	48	11	11	13	12	47
Mean	25.2	24.7	26.4	26.1	25.6	25.5	24.3	21.9	23.9	23.9
SD	2.75	2.93	.79	.90		2.07	2.97	5.85	7.64	

Mean of Both Treatments 24.8

26.4 with a treatment mean of 25.6. The mean scores of groups in Treatment II ranged from 21.9 to 25.5 with a treatment mean of 23.9. The grand mean of all groups was 24.8. These means indicate that groups in both treatments scored high on this test. The data appear to be skewed sharply to the left indicating mastery of recall of the principle by most subjects. The difference between the treatment means was 1.7 indicating little difference in performance on this test. All groups learned the principle well enough to achieve a high mean on this test that measured recall of the principle. There was a tendency for groups in Treatment I to score higher than groups in Treatment II. Means of three groups which had received Treatment I were higher than any mean achieved by groups which had received Treatment II.

The standard deviations of the groups within treatments show more diversity. Inspection of the standard deviations shows that the variation in scores received by subjects in groups which had learned with the concrete model (Treatment II) were greater than the scores of subjects in groups which had learned with the symbolic model. This diversity however, is a reflection of the difference in achievement, skewness of the scores, and a few extremely low scores of subjects in groups in Treatment II.

The smallest standard deviations reported were for Groups G and L both of which were in Treatment I. No subject in either of these two groups missed more than two problems. The largest standard deviation reported for a Treatment I group was in Group E. In this group one child scored 20, one child scored 21, one child scored 22, and the remainder scored 25 or above. Scores of subjects in Treatment II groups shows that the children tended to miss more problems and there are two extremely deviant scores. The two largest standard deviations were attained by Groups K and J and both of these groups had one extremely deviant score. The one subject who was absent on the testing day and took the test one week later was in Group K and solved no problems correctly. One child in Group J solved only six problems correctly. All other subjects scored 17 or higher. While there is skewness evident, reflecting high scores, the scores tend to be somewhat lower for groups in Treatment II. The two extremely deviant scores plus the number of lower scores in groups which had received Treatment II explain the apparent difference in standard deviations for groups in the two treatments.

B. Symbolic Transfer Test I

The purpose of the Symbolic Transfer Test I was to gather data concerning the ability of the groups of subjects to transfer their learning to solving untaught instances of the principle. Both groups used as aids in problem solving the model with which they had learned. The test was composed of twenty-three items and the score received was the number of items answered correctly. The distribution of scores, means and standard deviations achieved by the experimental groups on the Symbolic Transfer Test I are reported in Table 16.

The mean scores of groups in Treatment I ranged from 15.9 to 18.6 with a treatment mean of 17.7. The mean scores of groups in Treatment II ranged from 10.6 to 17.1 with a treatment mean of 14.2.

TABLE 16

DISTRIBUTION OF SCORES, MEANS AND STANDARD DEVIATIONS FOR EXPERIMENTAL GROUPS ON THE SYMBOLIC TRANSFER TEST I

	Treatment I					Treatment II				
	Instructional Group					Instructional Group				
Score	D	E	G	L	Total	A	F	J	K	Total
23		1	2	1	4	1		1		2
22	1		1	2	4			1	1	2
21	2	1	1	1	5		1	1	1	3
20	4	2	1		7	1			2	3
19	1		1	1	3	2	1		2	5
18		1		2	3	1		2	2	5
17		1	1	3	5		1	1	1	3
16		1	3	1	5	1		1	1	3
15	1	2	2	2	7			1		1
14									1	1
13						1	1	2		4
12		1			1	2	3			5
11	1	1			2					
10								1		1
9										
8	1				1					
7							1			1
6										
5										
4						1	1	1		3
3		1			1			1		1
2										
1									1	1
0						1	2			3
N	11	12	12	13	48	11	11	13	12	47
Mean	17.9	15.9	18.6	18.5	17.7	14.2	10.6	14.8	17.1	14.2
SD	4.57	5.42	3.12	2.73		7.01	7.16	6.26	5.52	

Mean of Both Treatments 15.9

The difference between the treatment means is 3.5 indicating that the groups which had learned with the symbolic model (Treatment I) did somewhat better than did the groups which had learned with the concrete model (Treatment II) on this test. The mean scores of three groups which were in Treatment I were higher than the mean score of any group in Treatment II.

Once again the same trend is seen in the standard deviations of the scores received by groups within treatments that was noted in the results of the Recall Test. The greatest standard deviation of any group in Treatment I was smaller than the smallest standard deviation of any group in Treatment II. This again is a reflection of the lower scores achieved by subjects in Treatment II groups. One subject in Group E (Treatment I) received a score of 3. One subject in Group D (Treatment I) received a score of 8. These two scores help explain the relatively larger standard deviations in these groups as all other scores of subjects in Treatment I were 11 or higher, and all but 3 additional scores were fifteen or higher. The distribution of scores of these groups tend to be skewed indicating high achievement.

A total of nine children in Treatment II groups scored less than eight; 2 in Group A, 3 in Group F, 2 in Group J, and 1 in Group K. The distribution of scores appear to be normal, but there are more low scores in groups in this treatment than there were in groups in Treatment I.

C. Symbolic Transfer Test II

The purpose of the Symbolic Transfer Test II was to measure the ability of the groups of subjects to transfer their learning to solving untaught instances of the principle when both groups used the same familiar concrete materials as aids in the problem solving. The test was identical to the Symbolic Transfer Test I and contained twenty-three items. The score received was the number of items answered correctly. The distribution of scores, means and standard deviations achieved by the experimental groups on the Symbolic Transfer Test II are reported in Table 17.

The mean scores received by groups in Treatment I ranged from 17.6 to 19.9 with a treatment mean of 18.4. The means received by groups in Treatment II ranged from 10.8 to 14.9 with a treatment mean of 13.3. The mean score of each group which had learned with the symbolic model was higher than the mean score of any group which had learned with the concrete model. The overall difference in the treatment means was 5.3 which appears to be large enough to indicate a real difference in ability to transfer learning to this type of test.

A comparison of the mean scores on this test with the mean scores on the Symbolic Transfer Test I shows that the grand mean of the Symbolic Transfer Test I was 15.9 and the Grand mean on the Symbolic Transfer Test II was 15.8, a small difference which indicates that the overall performance on the two tests was similar. However, the treatment mean for Treatment I groups for the Symbolic Transfer Test I was 17.7 and for the Symbolic Transfer Test II was 18.4. This

TABLE 17
DISTRIBUTION OF SCORES, MEANS AND STANDARD DEVIATIONS
FOR EXPERIMENTAL GROUPS ON THE SYMBOLIC TRANSFER TEST II

	Treatment I					Treatment II				
	Instructional Group					Instructional Group				
Score	D	E	G	L	Total	A	F	J	K	Total
23	4	3	1	1	9	1		1	2	4
22	1	1		2	4			1		1
21	1	1	3	4	9	1		1		2
20			1	1	2	1	1			2
19			1	2	3				1	1
18	1	2	1	1	5		1	1	1	3
17		1		1	2	1				1
16			2		2	2			1	3
15	1			1	2				2	2
14		2	2		4			1	1	2
13	1		1		2	1	1	2	1	5
12		1			1	1	3	2	1	7
11	1				1	2	2		1	5
10		1			1			2		2
9										
8							1			1
7										
6										
5										
4										
3										
2	1				1		1			1
1								2		2
0						1	1		1	3
N	11	12	12	13	48	11	11	13	12	47
Mean	17.6	17.9	18.0	19.9	18.4	14.5	10.8	13.1	14.9	13.3
SD	6.80	4.60	3.33	2.25		6.31	5.90	6.94	6.10	

Mean of Both Treatments 15.8

indicated that the Treatment I groups did somewhat better on the second symbolic transfer test than they did on the first symbolic transfer test. The opposite is seen in the case of the Treatment II groups. The mean of the Treatment II groups on the Symbolic Transfer Test I was 14.2 and the Symbolic Transfer Test II was 13.1. Children in Treatment II groups did not perform as well on the second transfer test as they did on the first.

The same trend in the variation of the groups' performances is seen in this test as was seen in the Recall and Symbolic Transfer Test I. The scores of children who had learned with the concrete model (Treatment II) varied over a wider range than did those who had learned with the symbolic model (Treatment I). This difference in variation of scores does not appear to be as great on this test as it was on the other two.

The difference in variation of scores is accounted for by the number of lower scores received by subjects in groups in Treatment II. Only one subject in a Treatment I group (Group D) achieved less than 10. Seven children in Treatment II groups received less than 10: 1 subject in Group A, 3 subjects in Group F, 2 subjects in Group J, and one subject in Group K.

One additional trend should be noted. On all three tests Group E received the lowest mean score of any group in Treatment I. On the two transfer tests Group F received the lowest score of any group in Treatment II. Both of these groups came from the same classroom which leads one to speculate on what outside factor other than treatment might have been affecting the scores on these two tests.

D. Concrete Transfer Test

The purpose of the Concrete Transfer Test was to measure the ability of the groups of children to demonstrate the learned principle on an unfamiliar concrete device. Their ability to do this was measured on two scales: seconds required, and number of trials required to complete the items on the test. The test was divided into two parts for convenience of administration. Separate data and analyses are available for each part as well as for the total test but the results of each part are in general agreement with the results for the total test so only data and analyses for the total time and number of trials required will be reported here. Data and analysis for the separate parts appear in Appendix G.

The distribution of scores, means and standard deviations achieved by the experimental groups on the Concrete Transfer Test: Total Number of Trials are reported in Table 18.

The mean number of trials required for completion of the items on the test for Treatment I groups ranged from 65.6 to 74.3 with a treatment mean of 69.5. The trials required by Treatment II groups ranged from 65.3 to 77.4 with a treatment mean of 71.1. The difference between the two treatment means is 1.6 trials which is small.

The variation of the groups' scores once again shows the same tendency that was shown by the other three tests. Three of the Treatment I groups' standard deviations was less than any standard deviation

TABLE 18
DISTRIBUTION OF SCORES, MEANS AND STANDARD DEVIATIONS OF EXPERIMENTAL GROUPS ON CONCRETE TRANSFER TEST: TOTAL NUMBER OF TRIALS

	Treatment									
	I					II				
	Instructional Group					Instructional Group				
Score	D	E	G	L	Total	A	F	J	K	Total
30- 39	1				1	1				1
40- 49	1	1		1	3	2	2			4
50- 59	2	5	2	4	13	1	3	4	5	13
60- 69	2	3	3	3	11	1	3	4	2	10
70- 79	2	1	3	2	8	3		1	3	7
80- 89	2		1	2	5		2	2		4
90- 99		1	1		2			1		1
100-109	1	1	2	1	5	1		1	1	3
110-119							1			1
120-129						1			1	2
130-139						1				1
N	11	12	12	13	48	11	11	13	12	47
Mean	68.8	65.6	74.8	68.7	69.5	77.4	65.3	70.0	71.6	71.1
SD	20.4	17.5	16.4	15.5		31.7	19.5	18.1	21.8	

Mean of Both Treatments 70.3

of any group in Treatment II. The number of trials required to successfully complete this test varied more among children who had learned with the concrete model than did those scores achieved by children who had learned with the symbolic model.

The distribution of scores, means and standard deviations achieved by the experimental groups on the Concrete Transfer Test: Total Time are reported in Table 19.

The mean number of seconds required for Treatment I groups to complete the test varied from 698.8 to 789.4 with a treatment mean of 732.8 seconds. The mean number of seconds required for Treatment II groups varied from 686.3 to 798.7 with a treatment mean of 756.3. The two treatment means differ by 23.5 seconds. There appears to be little difference in the time required by the treatment groups to successfully complete the items on the test.

However, the same trend previously noted is present in the variation of the two groups scores although somewhat less evident here. There is more variation apparent in the scores received by the children who had learned with the concrete model than in the scores received by children who had learned with the symbolic model.

II. Analysis of Data from Measures of Dependent Variables

Two sets of data analyses are reported. The first reports the examination of the data from all the tests considered collectively. This analysis yielded information relevant to the difference or similarity in overall learning (Hypothesis I).

TABLE 19
DISTRIBUTION OF SCORES, MEANS AND STANDARD DEVIATIONS OF EXPERIMENTAL GROUPS ON CONCRETE TRANSFER TEST: TOTAL TIME*

	Treatment									
	I Instructional Group					II Instructional Group				
Score	D	E	G	L	Total	A	F	J	K	Total
480- 539		1	1	1	3	1	2	1		4
540- 599	1			1	2	2	1	2	1	6
600- 659	4	3	5	1	13	2	3	1		6
660- 719	3	5		4	12	2		1	2	5
720- 779	1	1		2	4		1	1	2	4
780- 839			1	1	2	1	1	3	3	8
840- 899		1	1	2	4		1	1	2	4
900- 959	1	1	2		4	1		2	1	4
960-1019	1		1	1	3					
1020-1079							1	1	1	3
1080-1139										
1140-1199						1				1
1200-1259			1		1					
1260-1319						1				1
N	11	12	12	13	48	11	10**	13	12	46
Mean	711.3	698.8	789.4	731.6	732.8	772.8	686.3	767.4	798.7	756.3
SD	130.89	103.50	206.94	130.58		254.08	259.06	168.44	120.96	
	Mean of All Groups 744.5									

* Time reported in seconds

** Data missing for one subject

The second set of analyses involves data from each test considered separately. These analyses are concerned with the differences or similarities found in specific dimensions of learning (Hypotheses II-V).

A. Hypothesis I

A multivariate analysis of variance (Manova) using the mean scores of the four tests as the data for analysis, yielded information relevant to Hypothesis I:

> There are no significant differences in the learning of a selected mathematical principle between groups of children who have learned that principle using a meaningful concrete or a meaningful symbolic model.

Manova technique permitted examination of the overall relationship between the four measures of dependent variables. The tests were not measures of variables which were independent of each other but measures of different dimensions of the subjects' learning of the same mathematical principle. The dimensions that were considered were direct recall, transfer to symbolic instances of the principle using two different sets of aids, and transfer to a concrete representation of the principle. The results of the Manova appears in Table 20.

TABLE 20

MULTIVARIATE ANALYSIS OF VARIANCE OF MEAN SCORES
RECEIVED BY EXPERIMENTAL GROUPS ON ALL MEASURES
OF DEPENDENT VARIABLES

F-Ratio for multivariate test of equality of mean vectors = 4.378

Degrees of Freedom: 5 and 2

Not Significant, $p < 0.1964$

The F ratio obtained was 4.3785 which with 5 and 2 degrees of freedom result in a probability level of less than .1964. This level is greater than the level chosen for significance so the hypothesis was not rejected. The differences observed through inspection of the mean scores was not large enough to be significant statistically. There were no statistically significant differences in the overall learning of the mathematical principle between groups of children who had learned with a meaningful concrete or a meaningful symbolic model.

Not only was this study concerned with the overall learning of the mathematical principle, but also with the several dimensions of learning measured by the tests. To gather information relevant to the separate dimensions of learning, one-way analyses of variance (Anova) were computed using the mean scores of groups on each test as a basis of analysis. These analyses gave information relevant to Hypotheses II, III, IV, and V.

B. Hypothesis II

Mean scores obtained by the experimental groups on the Recall Test were analysed to give information relevant to Hypothesis II:

> There are no significant differences in the ability to recall instances of a selected mathematical principle between groups of children who have learned that principle using a meaningful concrete or a meaningful symbolic model.

The results of this Anova are reported in Table 21.

TABLE 21

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL TREATMENT GROUPS ON RECALL TEST

Source of Variation	Sum of Squares	df	Mean Square	F Ratio	P
Between	5.86	1	5.86	4.08	.090
Within	8.62	6	1.44		

The F ratio obtained was 4.08 which with 1 and 6 degrees of freedom give a probability of .090. This p is greater than that established for rejection so the hypothesis was not rejected. That there were no significant differences in this dimension of learning is not surprising. It was expected that groups in both treatments would perform well on this test and the means and standard deviations reported in Table 15 indicate that all groups did learn the principle. The fact that the p comes as close to the rejection level as it did might result from the larger variance in scores evident in groups which received Treatment II.

The next three hypotheses all deal with questions concerned with the transfer of learning from learned instances of the principle to solving problems of untaught instances of the principle. Hypotheses III and IV deal with transfer to symbolic representations. Hypothesis V deals with transfer to a concrete representation of the principle. It was anticipated that if real differences occurred in the learning of the principle, it would occur in the transfer portion of the dependent variables.

C. Hypothesis III

> There are no significant differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who have learned that principle using a meaningful concrete or a meaningful symbolic model when the subjects are permitted to use the model with which they learned.

Mean scores obtained by the experimental groups on the Symbolic Transfer Test I were analysed to give information relevant to this hypothesis. The results of this Anova are reported in Table 22.

TABLE 22

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL TREATMENT GROUPS ON SYMBOLIC TRANSFER TEST I

Source of Variation	Sum of Squares	df	Mean Square	F Ratio	P
Between	24.92	1	24.92	5.76	.053
Within	25.95	6	4.32		

The F ratio was 5.76 which with 1 and 6 degrees of freedom gives a probability of .053. This p value very closely approaches the level set for rejection. It might be anticipated that if the degrees of freedom had been larger, the F ratio would have had a p of less than .05. (Although use of individual scores does not meet the assumption of independence of scores which underlie use of the Anova technique, an Anova was computed on these scores and the results reporting significant differences appear in Appendix H .) Inspection of the mean scores of the experimental groups (Table 16) shows that the groups which had learned with the Symbolic Model (Treatment I) scored higher than those which had learned with the Concrete Model (Treatment II).

D. Hypothesis IV

> There are no significant differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who have learned using a meaningful concrete or a meaningful symbolic model when the children have familiar concrete materials to use as aids in the problem solving.

Mean scores obtained by the experimental groups on the Symbolic Transfer Test II were analysed to give information relevant to this hypothesis. The results of this Anova are reported in Table 23.

An F ratio of 22.27 was found which with 1 and 6 degrees of freedom gives a probability of less than .003. This p was less than the level set for rejection so the hypothesis was rejected. There were differences in the ability to solve problems of untaught

Table 23

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL TREATMENT GROUPS ON SYMBOLIC TRANSFER TEST II

Source of Variation	Sum of Squares	df	Mean Square	F Ratio	P
Between	50.60	1	50.60	22.27	.003**
Within	13.62	6	2.27		

instances of a mathematical principle between groups of children who had learned using a meaningful concrete or a meaningful symbolic model when the children had familiar concrete materials to use as aids in the problem solving. These differences were highly significant.

An inspection of the mean scores attained by groups within treatments (Table 17) indicates that groups who had received Treatment I scored higher on this test. Learning with a symbolic model enabled better transfer, as measured by this test, than did learning with a concrete model.

E. Hypothesis V

> There are no significant differences in the ability to demonstrate a learned mathematical principle on an unfamiliar concrete model between groups of children who have learned the principle with a meaningful symbolic or a meaningful concrete model.

Information concerning this hypothesis was obtained by administering the Concrete Transfer Test. Two scores were available for each individual for this test: total time required and total number of trials required. Mean scores of these two scales were analysed separately and the results of both of the analyses are relevant to this hypothesis.* The Anovas done on these two sets of scores are reported in Tables 24 and 25.

Both of these analyses yielded the same basic information. The F ratio obtained for the total time required was .557 which with 1 and 6 degrees of freedom result in a probability of less

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*Analyses of Variance on the Concrete Transfer Test, Parts I and II can be found in Appendix G.

TABLE 24

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL TREATMENT GROUPS ON CONCRETE TRANSFER TEST: TOTAL TIME

Source of Variation	Sum of Squares	df	Mean Square	F Ratio	P
Between	1105.20	1	1105.20	.557	.484
Within	11915.6	6	1985.93		

TABLE 25

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL TREATMENT GROUPS ON CONCRETE TRANSFER TEST: NUMBER OF TRIALS

Source of Variation	Sum of Squares	df	Mean Square	F Ratio	P
Between	5.10	1	5.10	.254	.633
Within	120.77	6	20.13		

*Analyses of Variance on the Concrete Transfer Test, Parts I and II can be found in Appendix G than .484. The F ratio obtained for the total number of trials required was .254 which with 1 and 6 degrees of freedom result in a probability of .633. Neither of these probabilities approaches the level necessary for rejection so the hypothesis was not rejected. It appears from both these analyses and inspection of the means attained by the groups (Table 18) that there was very little difference in the ability to demonstrate the principle on an unfamiliar concrete device between groups of children who had learned with a meaningful concrete or symbolic model.

III. Another Hypothesis (Hypothesis III)

> There are no significant differences in the ability to demonstrate a mathematical principle on an unfamiliar concrete model between groups of children who had instruction in the principle with a meaningful concrete or a meaningful symbolic model and groups of children who received no instruction in that principle.

In order to gather data that would enable acceptance or rejection of this hypothesis, the Concrete Transfer Test was administered to four control groups (Treatment III) which had not participated in the instructional portion of the study. This data plus the data from the experimental groups' performance on the same test yielded information concerning Hypothesis VI. The distribution of scores, means and standard deviations of all treatment groups for the total time and total number of trials required to complete the items on this test are shown in Table 26 and 27.*

The mean number of seconds required for the groups in Treatment III to complete the test ranged from 687.1 to 845.4 with a treatment

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*The distribution of scores, means and standard deviations for Parts I and II, and the analyses of variance performed with the means can be found in Appendix G.

TABLE 26

DISTRIBUTION OF SCORES, RANGES, MEANS AND STANDARD DEVIATIONS FOR GROUPS ON THE CONCRETE TRANSFER TEST: TOTAL NUMBER OF TRIALS

Treatment I

Score	Instructional Group				Total
	D	E	G	L	
20- 29					
30- 39	1				1
40- 49	1	1		1	3
50- 59	2	5	2	4	13
60- 69	2	3	3	3	11
70- 79	2	1	3	2	8
80- 89	2		1	2	5
90- 99		1	1		2
100-109	1	1	2	1	5
110-119					
120-129					
130-139					
N	11	12	12	13	48
Mean	68.8	65.5	74.8	68.7	69.5
SD	20.37	17.55	16.55	15.48	

Treatment II

Score	Instructional Group				Total
	A	F	J	K	
20- 29					
30- 39	1				1
40- 49	2	2			4
50- 59	1	3	4	5	13
60- 69	1	3	4	2	10
70- 79	3		1	3	7
80- 89		2	2		4
90- 99			1		1
100-109	1		1	1	3
110-119		1			1
120-129	1			1	2
130-139	1				1
N	11	11	13	12	47
Mean	77.4	65.3	70.0	71.6	71.1
SD	31.75	19.51	18.10	21.77	

TABLE 26 (con't.)

DISTRIBUTION OF SCORES, RANGES, MEANS AND STANDARD DEVIATIONS FOR GROUPS ON THE CONCRETE TRANSFER TEST: TOTAL NUMBER OF TRIALS

Treatment III

Score	Instructional Group				Total
	B	C	H	I	
20- 29	1				1
30- 39	1				1
40- 49		2	2	3	7
50- 59	2	2	2	3	9
60- 69	1	2		2	5
70- 79	2	1	2	3	8
80- 89	1	1		1	3
90- 99			2	1	3
100-109			3		3
110-119		1			1
120-129		1			1
130-139	1				1
N	9*	10	11	13	43
Mean	64.9	72.4	77.4	63.8	69.6
SD	31.87	25.89	23.14	16.38	

Mean of All Treatments 70.1

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*Data missing on one subject.

TABLE 27

DISTRIBUTION OF SCORES, MEANS AND STANDARD DEVIATIONS FOR ALL TREATMENT GROUPS ON CONCRETE TRANSFER TEST: TOTAL TIME*

Treatment I

	Instructional Group				
Score	D	E	G	L	Total
300-359					
360-419					
420-479					
480-539		1	1	1	3
540-599	1			1	2
600-659	4	3	5	1	13
660-719	3	5		4	12
720-779	1	1		2	4
780-839			1	1	2
840-899		1	1	2	4
900-959	1	1	2		4
960-1019	1		1	1	3
1020-1079					
1080-1139					
1140-1199					
1200-1259			1		1
1260-1319					
N	11	12	12	13	48
Mean	711.3	698.8	789.4	731.6	732.8
SD	130.89	103.50	206.94	130.58	

Treatment II

	Instructional Group				
Score	A	F	J	K	Total
300-359					
360-419					
420-479					
480-539	1	2	1		4
540-599	2	1	2	1	6
600-659	2	3	1		6
660-719	2		1	2	5
720-779		1	1	2	4
780-839	1	1	3	3	8
840-899		1	1	2	4
900-959	1		2	1	4
960-1019					
1020-1079		1	1	1	3
1080-1139					
1140-1199	1				1
1200-1259					
1260-1319	1				1
N	11	10*	13	12	46
Mean	772.8	686.3	767.4	798.7	756.3
SD	254.08	259.06	168.44	120.96	

TABLE 27 (con't.)

DISTRIBUTION OF SCORES, MEANS AND STANDARD DEVIATIONS FOR ALL TREATMENT GROUPS ON CONCRETE TRANSFER TEST: TOTAL TIME*

	Treatment III Instructional Group				
Score	B	C	H	I	Total
300-359	1				1
360-349					
420-479					
480-539					
540-599	1	1	1	5	8
600-659		1	1	1	3
660-719	2	2	2	2	8
720-779	4	2	1	2	9
780-839		1		2	3
840-899			2		2
900-959		2	1	1	4
960-1019					
1020-1079			1		1
1080-1139			2		2
1140-1199		1			1
1200-1259	1				1
1260-1319					
N	9**	10	11	13	43
Mean	727.0	786.3	845.4	687.1	761.4
SD	232.62	172.41	192.99	116.17	

Mean of All Treatments 750.2

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* Time reported in seconds

** Data missing on one subject

mean of 761.4. The number of seconds required for the Treatment II groups ranged from 686.3 to 798.7 with a treatment mean of 756.3 The number of seconds required for Treatment I groups ranged from 698.8 to 789.4 with a treatment mean of 732.8. The treatment means indicate that there was approximately a forty second difference required for completion of the test among the three treatment groups which is no practical difference. The same trend is evident in the number of trials required. An inspection of these data suggests that all groups whether they had received instruction in the principle or not performed approximately the same on this test which required demonstration of the principle on an unfamiliar concrete device. There was little or no transfer of learning evidenced by the groups which had learned with the concrete or symbolic model. If there had been, these groups should have performed at a higher level on this test than groups which had never been exposed to instruction in the principle.

To check this observation, two Anovas were computed using the mean scores of all groups on total time and total trials required to complete the test. These analyses are shown in Tables 28 and 29.

The analysis done on the total time required resulted in an F ratio of .319 which with 2 and 9 degrees of freedom gives a probability less than .734. The analysis done on the total trials resulted in an F ratio of .113 which with 2 and 9 degrees of freedom gives a probability of less than .895. These two F ratios have probabilities which do not approach the level set for rejection so the hypothesis was not rejected. These analyses confirmed the observation of the

TABLE 28
ANALYSIS OF VARIANCE OF MEAN SCORES OF CONTROL AND EXPERIMENTAL GROUPS ON CONCRETE TRANSFER TEST: TOTAL TIME

Source of Variation	Sum of Squares	df	Mean Square	F Ratio	p
Between	1866.49	2	933.25	.319	.734
Within	26291.72	9	2921.30		

TABLE 29
ANALYSIS OF VARIANCE OF MEAN SCORES OF CONTROL AND EXPERIMENTAL GROUPS ON CONCRETE TRANSFER TEST: TOTAL NUMBER OF TRIALS

Source of Variation	Sum of Squares	df	Mean Square	F Ratio	p
Between	6.16	2	3.08	.113	.895
Within	245.62	9	27.29		

mean scores. There were no differences in the ability to demonstrate a mathematical principle on an unfamiliar concrete device between groups of children who had instruction with a meaningful concrete model, a meaningful symbolic model or no instruction at all.

IV. Summary

Table 30 shows a summary of the analyses results. In general the data and the analyses performed with it indicate:

The groups that learned with the symbolic model did better in overall learning of the principle, although not significantly so, than did groups which had learned with the concrete model.

The groups which had learned with the symbolic model also performed better but not significantly so, on a test which measured direct recall. The groups which had learned with the symbolic model performed better on two tests of symbolic transfer. These differences in performance barely missed significance on one test of symbolic transfer and were highly significant on the other. There were no significant differences in performance on the test of concrete transfer.

There were no significant differences in performance on the test of concrete transfer between groups which had learned with the symbolic model, concrete model, or had received no instruction in the principle.

TABLE 30
SUMMARY OF STATISTICAL ANALYSES
RELEVANT TO THE HYPOTHESES

Hypothesis	Dimension of Learning	Analysis	F ratio	p
I	Overall	Manova	4.3785	.1964
II	Recall	Anova	4.084	.090
III	Symbolic Transfer I	Anova	5.761	.053
IV	Symbolic Transfer II	Anova	22.276	.003**
V	Concrete Transfer[a]			
	Time	Anova	.557	.484
	Trials	Anova	.254	.633
VI	Concrete Transfer[b]			
	Time	Anova	.319	.734
	Trials	Anova	.113	.895

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[a] Experimental Groups

[b] Experimental and Control Groups

IV

SUMMARY, CONCLUSIONS AND DISCUSSION

I. Summary

A. Problem and Hypotheses

Although there is agreement on what mathematical principles should be taught in the elementary school, there is little agreement upon how learning environments can best be structured to facilitate the learning of those principles. Most mathematics educators agree that meaningful learning is better than nonmeaningful learning, but there is little knowledge about what component of the mathematics curriculum facilitates meaningful learning. The major problem of this study was to examine the effects upon learning of one specific portion of the mathematics curriculum: i.e. the relative effectiveness of a meaningful concrete and a meaningful symbolic model in facilitating the learning of a mathematical principle.

The components of the mathematics curriculum are teachers, learners, instructional activities and curriculum materials (DeVault, 1966). In order to gather data that would give information relevant to the major problem of the study, curriculum components of teachers, learners, and that portion of instructional activities and materials not directly related to the independent variable were carefully controlled. The independent variable of the study was the model used to give meaning to a selected mathematical principle.

The following hypotheses were examined:

Hypothesis I: There are no significant differences in the learning of a mathematical principle between groups of children who have learned that principle using a meaningful symbolic or a meaningful concrete model.

Hypothesis II: There are no significant differences in the recall of instances of a selected mathematical principle between groups of children who have learned that principle using a meaningful concrete or a meaningful symbolic model.

Hypothesis III: There are no significant differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who have learned that principle using a meaningful symbolic or a meaningful concrete model when they used as aids in problem solving that model with which they learned.

Hypothesis IV: There are no significant differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who have learned that principle using a meaningful symbolic or a meaningful concrete model when they have familiar concrete materials to use as aids in problem solving.

Hypothesis V: There are no significant differences in the ability to demonstrate a mathematical principle on an unfamiliar concrete model between groups of children who have learned the principle using a meaningful symbolic or a meaningful concrete model.

Hypothesis VI: There are no significant differences in the ability to demonstrate a mathematical principle on an unfamiliar concrete model between groups of children who have had instruction in the principle with a meaningful concrete or a meaningful symbolic model and groups of children who have received no instruction in that principle.

B. Experimental Design, Analyses and Results

The Oregon, Wisconsin Elementary School second grade served as the population for this study. Subjects who did not measure at or above criterion level on a Qualifying Examination were eliminated.

The remaining subjects in each of the six second grade classrooms were randomly divided into two groups, and the resulting twelve groups were randomly assigned to one of three treatments. Groups in Treatment I and Treatment II received instruction concurrently in the same mathematical principle from the same teacher. Groups receiving Treatment I learned the principle using a meaningful symbolic model and groups receiving Treatment II learned the principle using a meaningful concrete model. Groups assigned to Treatment III did not participate in the instructional portion of the study but served as control groups for one dependent variable measure. Data collected from Treatment III groups were relative to Hypothesis VI only.

At the end of the instructional period learning was evaluated by tests that measured the dependent variables: The Recall Test, the Symbolic Transfer Test I, Symbolic Transfer Test II, and the Concrete Transfer Test. The mean scores of groups in Treatment I were generally higher than the mean scores of groups in Treatment II on the Recall Test, the Symbolic Transfer Test I and the Symbolic Transfer Test II. There were no consistent differences in the mean time or mean number of trials required for groups receiving Treatment I or Treatment II to successfully complete the Concrete Transfer Test. No consistent differences were evident in the performance of the experimental groups and the control groups on the Concrete Transfer Test.

To determine the significance of the differences in mean scores, statistical analyses were done and a summary of the results of these analyses is reported in Table 31. The multivariate analysis of

TABLE 31
SUMMARY OF STATISTICAL ANALYSES
RELEVANT TO THE HYPOTHESES

Hypothesis	Dimension of Learning	Analysis	F ratio	P
I	Overall	Manova	4.3785	.1964
II	Recall	Anova	4.084	.090
III	Symbolic Transfer I	Anova	5.761	.053
IV	Symbolic Transfer II	Anova	22.276	.003**
V	Concrete Transfer[a]			
	Time	Anova	.557	.484
	Trials	Anova	.254	.633
VI	Concrete Transfer[b]			
	Time	Anova	.319	.734
	Trials	Anova	.113	.895

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[a] Experimental Groups
[b] Experimental and Control Groups

variance which provided information relative to Hypothesis I yielded an F ratio too small to give a probability level which would allow rejection of the hypothesis. One-way analyses of variance of the mean scores received by the groups on the various tests of learning yielded information relative to the other hypotheses. The probability of the F ratios relative to Hypotheses II, V, and VI were too large to be significant so these hypotheses were not rejected. Hypothesis III also was not rejected even though the F ratio had a probability close to the rejection level. The F ratio relative to Hypothesis IV had a probability of less than .05 and this hypothesis was rejected.

II. Conclusions and Discussion

A. The Major Problem and Hypothesis I

The major problem of this study was to determine the relative effectiveness of a meaningful concrete and a meaningful symbolic model in facilitating learning of a specific mathematical principle. For this study learning was characterized as having the dimensions of recall, transfer of learning to solving problems of the learned principle stated in untaught symbols, and transfer of learning to demonstration of the principle on an unfamiliar concrete model. These dimensions of learning were measured by the four tests: Recall Test, Symbolic Transfer Test I, Symbolic Transfer Test II, and the Concrete Transfer Test.

These tests were administered to the experimental groups at the end of the instructional portion of the study. On the Recall Test, the Symbolic Transfer Test I, and the Symbolic Transfer Test II the mean scores received by the groups who had received Treatment I (Symbolic Model) were generally higher than the groups who had received Treatment II (Concrete Model). Differences in performance on the Concrete Transfer Test among groups within treatments were inconsistent .

To determine the significance of the differences in performance on the various tests which measured learning, the mean scores received by the experimental groups were used as the basis of a multivariate analysis of variance. This technique enabled consideration of the results of the four tests collectively. Based on the resulting F ratio, the probability was too high to warrant rejection so the hypothesis that there were no significant differences in the learning of a mathematical principle between groups of children who had learned that principle using a meaningful symbolic or a meaningful concrete model could not be rejected.

Piaget and his followers would have us believe that learning by young children is facilitated by physical interaction with the environment: i.e. with concrete models. Studies done with subjects the age of the children in this study, (Aurich, 1963; Hollis, 1964; Crowder, 1965; Lucas, 1966; Ekman, 1966; Dawson and Ruddell, 1955b; Norman, 1955; Howard, 1950) who learned with or without concrete models, also give support to this belief. Several factors might explain the results of this study which appear to contradict the results of other studies. In many of the studies concerned with the use of concrete materials (Ekman, 1966; Dawson and Ruddell, 1955b; Norman, 1955; Howard, 1950) little was done to make the learning of mathematical principles equally meaningful for all experimental treatments. A close inspection of such studies leads one to believe that the subjects who learned with concrete models were able to learn better because the model demonstrated where the principle to be learned fitted into the structure of mathematics. The principle was taught meaningfully. Little evidence is presented that subjects who learned without concrete models were aided in understanding the relationship between principles and structure that makes learning meaningful. The independent variable that appeared to be under consideration in those studies was the presence or absence of meaningful learning, as much as or more than the use of concrete or symbolic models.

In this study the principle was demonstrated in both treatments meaningfully. In Treatment I, the principle was demonstrated in relation to the structure of mathematics which the learners and acquired previously and could use in symbolic terms. In Treatment II, the principle was made meaningful by demonstration with the concrete model. Therefore, meaning was present in both treatments and the true independent variable was the use of a concrete or symbolic model. When results of the tests which measured learning were considered collectively no significant differences in learning were found.

These results are not in conflict with the presence or the ordering of Piaget's developmental stages. Young children need to interact with concrete representations of ideas before they can use symbols to express those ideas. In this study, the children who learned with the symbolic model were basing this learning on their own conception of number, addition, and equality. This conception was at the symbolic stage as evidenced by the Qualifying Examination. Piaget would say this conception had to be based on physical interaction with the principle and there is no reason to believe this was not so. These children lived in an environment in which these principles are prevalent. They had participated in a mathematics program which emphasized manipulation of concrete objects. Since they had had these pre-symbolic experiences, they were able to move ahead in manipulation of the symbols. The making of the symbolic model meaningful in the way in which it was done ensured that children could see the relation to what they had previously experienced.

This study does give credence to the belief that children of this age can learn with symbols when those symbols are presented meaningfully in relation to knowledge they already possess. It raises a question as to whether the age range attached to the beginning of the operational stage by Piaget accurately describes the subjects in this study.

It should be pointed out that the Treatment II groups (Concrete Model) spent less time in learning the mathematical principle. Treatment I groups received instruction in the principle for thirteen periods while Treatment II Groups received instruction for a little more than twelve periods. Time taken for the learning of the additional ideas required for the learning of the principle by Treatment II groups did not prevent them from learning that principle as well as Treatment I groups.

It is difficult within the confines of this study to assess what the children who worked with the concrete model actually learned. The results of the Manova indicate that they did not learn the principle any better than did the children who learned with the symbolic model. However, it would be interesting and useful to know what they gained in their understanding of what a number is, about addition, and about equality. These ideas were redefined for them throughout the course of the instructional period during which the concrete model was used. What and how much was learned in addition to the principle is not known. However, it seems reasonable to conclude that a dimension was added to their understanding of number, addition and equality.

B. Other Hypotheses

Learning is multi-dimensional. Each dependent variable measure was concerned with a dimension of learning and each was analyzed separately in an attempt to gain information about the effects of using a meaningful concrete or symbolic model on the specific dimension of learning under consideration.

1. Recall of the Principle

The dimension of learning measured by the Recall Test was specific recall of instances of the principle, stated in symbols, exactly as it was represented during instruction. The scores on this test indicate that using either the symbolic model or the concrete model enabled most subjects to recall the mathematical principle. The mean scores achieved by Treatment I groups were generally higher than those achieved by Treatment II groups but these differences were small.

To determine the significance of the differences, the mean scores of Treatment I and Treatment II groups were used as a basis for a one-way analysis of variance. The probability of the F ratio was larger than the level of significance decided upon so the hypothesis was not rejected that there were no significant differences in the recall of instances of a selected mathematical principle between groups of children who had learned that principle using a meaningful concrete or a meaningful symbolic model. Use of either model during the instructional period resulted in the ability to recall exact representations of the mathematical principle. Both models facilitated this type of learning.

2. Transfer of the Learning of the Principle

One purpose of any learning of mathematical principles is to develop the ability to solve problems involving that principle, the solutions to which have not been specifically taught: that is the ability to generalize or to transfer learning. The other dimensions of learning considered in this study deal with transfer of learning. In the three tests which measured transfer, children were required to either solve problems which were untaught symbolic instances of the principle or to demonstrate the principle on an unfamiliar concrete model. In the first two tests symbolic representations of the principle were used. In the third test, the principle could be demonstrated concretely.

a. Symbolic Transfer I

Learning with which model better enabled the groups of learners to generalize the model's applicability to solving problems which were untaught symbolic instances of the learned principle? This dimension of learning was measured by the Symbolic Transfer Test I. The mean scores for all four Treatment I groups on this test were higher than three of the four mean scores of Treatment II groups. The groups of children who had learned with the symbolic model did better than the groups of children who had learned with the concrete model.

In order to determine the significance of the differences in performance on this test, the mean scores of the groups within treatments were used as the basis for a one-way analysis of variance. The probability of the F ratio was .053. The hypothesis that there are no significant differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who had learned that principle using a meaningful symbolic or a meaningful concrete model when they used as aids in problem solving that model with which they learned, was rejected. This decision to reject the hypothesis was made because the .05 level was so nearly reached even though the vigorous test involving means was used rather than individual scores. (When individual scores were used as the basis of analysis, a significant F ratio resulted. The results of this Anova are in Appendix H.)

A confounding factor in the results of this test is the transfer required by the different treatment groups. A closer look discloses that in order to solve the problems, the transfer required for the groups which had received Treatment I was less than the transfer required for groups receiving Treatment II.

The mathematical content of the test was ordered pairs of numbers whose products were equal to the numbers eleven through sixteen. Children had to solve problems involving this content by filling in blanks with appropriate numbers to complete the set. In order to do this, children had to: 1) recognize all the numbers, 2) represent the numbers with the model, and 3) use the model to solve the problems.

Subjects in both treatments could recognize the numbers. This was determined by the Qualifying Examination. Representation of the numbers by groups which had learned with the symbolic model could be done by placing known examples (numerals) in the correct place in their model. However, the model used by groups in Treatment II had no representation of numbers larger than ten. Before the model could be used to solve the problems, the larger numbers had to be exemplified by placing two rods end to end. This required transfer by children in these groups. After the larger numbers were exemplified with the models, the models had to be used to solve the problems. This problem solving was new to all groups.

In reality it appears that groups which had learned with the concrete model were required to make two major transfers (exemplifying larger numbers and using the model to solve problems) while groups which had learned with the symbolic model were required to make only one major transfer (using the model to solve problems).

b. Symbolic Transfer II

Learning with which model better enabled the groups of subjects to transfer what they had learned to solving problems which were untaught instances of the learned principle when familiar concrete materials could be used as aids? This dimension of learning was measured by the Symbolic Transfer Test II. The mean score of each group which learned with the symbolic model (Treatment I) was higher than any mean score of any group which had learned with the concrete model (Treatment II).

In order to determine the significance of the differences in performance on this test, the mean scores of groups were used as a basis for a one-way analysis of variance. The F ratio found resulted in a probability of less than .003. Not only was there an observed difference in the performance of the treatment groups on this test, the difference in performance was significant beyond the .01 level and the hypothesis of no difference in the ability to transfer learning as measured by this test was rejected. There were significant differences in the ability to solve problems of untaught instances of a mathematical principle between groups of children who had learned that principle using a meaningful symbolic model or a meaningful concrete model when they had familiar concrete materials to use as aids in problem solving. Children who had learned with the symbolic model performed significantly better on this test than did children who had learned with the concrete model. Once again, it must be pointed out that there was more transfer required for those who had learned with the concrete model. All experimental subjects had, previous to the study, probably used counters (the aids used in this test) as a model of addition and had related addition to standard symbolism. The symbolic model of the principle in this study was more closely related to what children had done previously with the counters than was the concrete model.

The data and analyses from the two symbolic transfer tests considered together leads to the conclusion that groups of children who had learned with the symbolic model were better able to solve problems which were untaught symbolic instances of a mathematical principle than were groups of children who had learned with a concrete model. Learning with a symbolic model facilitated transfer of this type better than did learning with a concrete model.

One reason that mathematics is a powerful tool is the wide applicability of principles that can be symbolized. This symbolization in turn makes it possible to use the principles in new and different ways. Symbolization enables generalized application. This may be what happened in the case of the children who learned with the symbolic model. Meaningful teaching of the mathematical principle with the symbolic model might have enabled the learner to acquire the tools to apply this principle to solving problems that had not been met before. Meaningful concrete models, at least in the case of this portion of this study, were not as effective in providing the learners with the tools to solve unlearned problems.

c. Concrete Transfer

Does learning with a meaningful symbolic model or a meaningful concrete model of a mathematical principle better enable groups of children to demonstrate that principle on an unfamiliar concrete model? The Concrete Transfer Test measured this dimension of learning. Inspection of the mean scores of the various groups indicates that the difference in the mean number of trials or the mean time required to complete the test did not consistently favor groups in either treatment. These mean scores were used as the basis for two one-way analyses of variance to determine the significance of the differences. The probabilities of both F ratios were greater than the level set for rejection so the hypothesis was not rejected. There indeed were no significant differences in ability to transfer learning to the demonstration of a learned principle on an unfamiliar concrete device.

It had been anticipated that performance on this test would be somewhat higher for those children who had learned with the concrete model. The transfer required on this test appeared to be more closely related to the concrete model than to the symbolic model, because with both the balance beam and the Cuisenaire Rods, the principle could be demonstrated with concrete objects. Little symbolism was involved.

However, the children who had learned with the concrete model were able to demonstrate the principle on the balance beam no better than children who had learned with the symbolic model. It appeared that during the test, most children solved the problems by a trial and error method and few actually discovered the relationship between what they had learned and the balance beam.

The test was also administered to four groups of children (Treatment III) who had received no instruction in the principle. An inspection of the mean scores of all groups indicates no consistent difference in time or number of trials required to complete the test.

In order to determine the statistical significance of the observed similarity in performances, two one-way analyses of variance were computed using mean scores of all groups on both time and number of trials required to complete the test. Neither of the probabilities of the F ratios found were small enough to warrant rejection of the hypothesis so the hypothesis was not rejected. There were no significant differences in the ability to demonstrate a mathematical principle on an unfamiliar concrete model between groups of children who had instruction in the principle with a meaningful concrete or a meaningful symbolic model and groups of children who had received no instruction in that principle.

Children who had received instruction in the principle and who had learned the principle well enough to score high on a test of recall were unable to transfer that learning to demonstrating the principle on an unfamiliar concrete model. This conclusion must be reached because children who did not know the principle (Treatment III) performed as well on the Concrete Transfer Test as did the children who had participated in the experimental treatments (Treatments I and II). Even when teaching is meaningful, transfer is not automatic. Children in Treatments I and Treatment II were unable to see the relation of the principle they had learned to a new concrete device which was a model of that principle.

Mathematics is useful in a variety of ways to order the environment. Typically, however, the applications of the principles of mathematics to this ordering of the environment are not taught except in a limited way. Children are not helped to use mathematics outside of mathematics classes and as a result may not anticipate that mathematical principles can be applied in a variety of ways. The results of this test appear to be a reflection of that fact.

III. Limitations of the Study

Learning is multi-dimensional. Four dimensions of learning were measured in this study: recall and three aspects of transfer. It would have been useful to have measured at least one additional dimension: retention. Pragmatic reasons prevented its measurement. The second grade mathematics program of the school included the teaching of the selected mathematical principle. It was decided not to ask the classroom teachers to restrict the teaching of the mathematics program during the weeks following the experimental study and any teaching of the selected principle could have confounded the results of any testing of retention of learning.

The order of administering the two symbolic transfer tests should have been reversed for half the groups. The tests were identical in format and it would have been possible for subjects to have retained answers to specific problems from the first test to the second. In observing children taking the Symbolic Transfer Test II, there were no overt actions that would indicate children had retained specific answers to problems. The test was administered to small groups of children and they were watched while they solved the problems. All children used the counters on problems which did not involve one as a factor and many children used them on all problems. This indicates that specific answers, at least, were not often recalled.

The amount of time permitted for children receiving Treatment II to learn the concept of number, equality, and addition exemplified by the concrete model may have been insufficient. There is little evidence concerning how long it takes a second grade child to learn new mathematical concepts. The period allowed for doing this in this study may have been long enough to enable a child to use the knowledge to learn the mathematical principle well enough to recall it, but now long enough to enable effective transfer of the principle. It was felt by the teacher and the study director that only near the end of the instructional period, were the subjects in Treatment II grasping more completely the concept of number exemplified by the concrete model. What effect the lower level of understanding of the concept of number possessed by the groups receiving Treatment II as compared to groups in Treatment I is unknown. It may be assumed that a longer period of time was needed with the concrete model prior to the instructional period for the fullest exploitation of this experimental treatment.

The Symbolic Transfer Tests were somewhat biased in favor of the groups who learned the principle using the symbolic model. There was more similiarity between the symbolic model these groups used and the symbolic transfer tests than there was between the concrete model and the tests. This seems to be a limitation of many studies involving the use of symbolism and/or concrete objects. How is what children learn when they work with things other than symbols accurately assessed? This problem has not been solved and represents a major limitation of this study. An attempt was made to compensate for this by including a test which appeared to be more closely related to the concrete model than to the symbolic model (the Concrete Transfer Test). However, this attempt was not effective as the learning required to demonstrate the principle on the balance beam was not transfered by groups in either treatment.

IV. Implications

A. Implications for Further Research

In order for knowledge in a field to advance, any experimental study should be followed by two basic types of research studies. One basic type involves modified replication of the original study which will confirm or deny the findings and permit generalization of the findings to other populations. The other basic type involves building on the findings of the original study which permits an extension of knowledge concerning the area under investigation; in this case the relative effectiveness of concrete and symbolic models.

1. Replication Studies

The same basic study should be conducted again using the same mathematical principle, models, and the same age children. Prior to the beginning of instruction, children in all experimental groups should receive intensive instruction with the concrete model so that they would have a deeper understanding of the definition of number, addition and equality as exemplified by the model.

It would be useful to know if learning with one or the other model permitted learning applications of the principle more easily. To this end the Concrete Transfer Test could be modified. The amount of time it takes various groups of children to learn to balance the beam could be ascertained.

An additional analysis of the data resulting from such a study should be made. Observation of the scores on the various tests showed greater variation in response for those who had learned with the concrete model than for those who had learned with the symbolic model. Is this a statistically significant difference? Does the greater variability of scores produced by the use of the concrete model indicate that such models provide more adequately for individual differences than does use of a symbolic model? This question should be studied.

In order to permit generalization of these findings to a wider area of mathematics learning, this study should be replicated using different mathematical principles as the focus of instruction. Principles which are concerned with operations on whole numbers would be particularly appropriate.

The study should be replicated using control groups as a check on what is learned. Groups within the study should be pretested; receive one of three treatments; and then be post-tested. In this study control groups were used for only one measure of a dependent variable. The non-standard symbolic statement of the mathematical principle was included on three measures of dependent variables and this specialized symbolism prohibited solving of problems by subjects who had not participated in the instructional portion of the study. In a pilot study it was ascertained that six children the age of the subjects in this study could not solve problems such as were on the tests. To overcome this problem a mathematical principle stated in standard symbolism should be used as the focus of instruction of another study. A principle such as subtraction of whole numbers would be appropriate for young children.

It would be useful to know the progress made in learning a mathematical principle of children who learn with concrete or symbolic models. To ascertain this, a modified replication of this study could be made using the same basic design. Evaluation of what was learned could be made at periodic intervals as the subjects' learning processes toward mastery of the principle. Such a study would provide information about the rate of learning permitted by the two models.

2. Studies Which Extend Knowledge

The studies of transfer of learning suggest that diversity of experience improves the ability to transfer learning. Such diversity could be provided by having children use multiple concrete or symbolic models of a single mathematical principle as they learn. The relative effectiveness of using three models, two models and one model should be assessed. The principle of prime numbers might be investigated. Concrete models which exemplify this principle could be the Cuisenaire Rods; peg boards, utilizing the cardinal number of a set; and blocks which illustrate that prime numbers can be arranged only in one row while other numbers can be arranged in more than one row or column.

Are concrete models more effective in aiding learning of younger children than of older children? A study should be done with children in the intermediate grades using more advanced principles such as the addition of fractions.

Should concrete models be used exclusively for a time and then followed by the teaching of the symbolism? Should concrete models and symbolism be used concurrently, or should symbolism be used first? A study which investigates this problem could be designed to vary the order of use of concrete models and symbolism. Subjects in one treatment could spend the first portion of the study with symbolism and then spend the second portion of the study with concrete models. Subjects in another treatment would experience concrete models first and then symbolic models. In a third treatment, subjects would experience concrete and symbolic models concurrently.

B. Implications for the Teaching of Mathematics

The major conclusion of the study is that there were no significant differences in the overall learning of a selected mathematical principle between groups of children who had learned using a meaningful concrete or a meaningful symbolic model. Symbols can be used as models for teaching mathematical principles when they are related in a meaningful way to the structure of mathematics. Symbols can have meaning for young children when the symbols are related to mathematical principles the children know. Therefore, teaching with symbols is seemingly as effective as teaching with concrete models if provision for making the symbols meaningful is included.

Children can also learn mathematical principles using a meaningful concrete model. This study showed that children did learn overall as much with a concrete model in a shorter amount of time than did children who learned using the symbolic model. It is hypothesized that children in this study who learned with the concrete model also learned other mathematical ideas that were not measured by the evaluation instruments. Therefore, teachers can safely include concrete models in the mathematics curriculum of young children knowing that the amount of time spent with such models results in measurable learning of mathematical principles and probably also results in some non-measurable aspects of mathematical learning.

One of the most significant findings of the study was that children who had learned with a symbolic model alone, could transfer that learning better to solving problems stated in symbols, than could children who had learned with a concrete model. Making mathematical principles meaningful through the use of symbolic models alone may be a very powerful instructional technique.

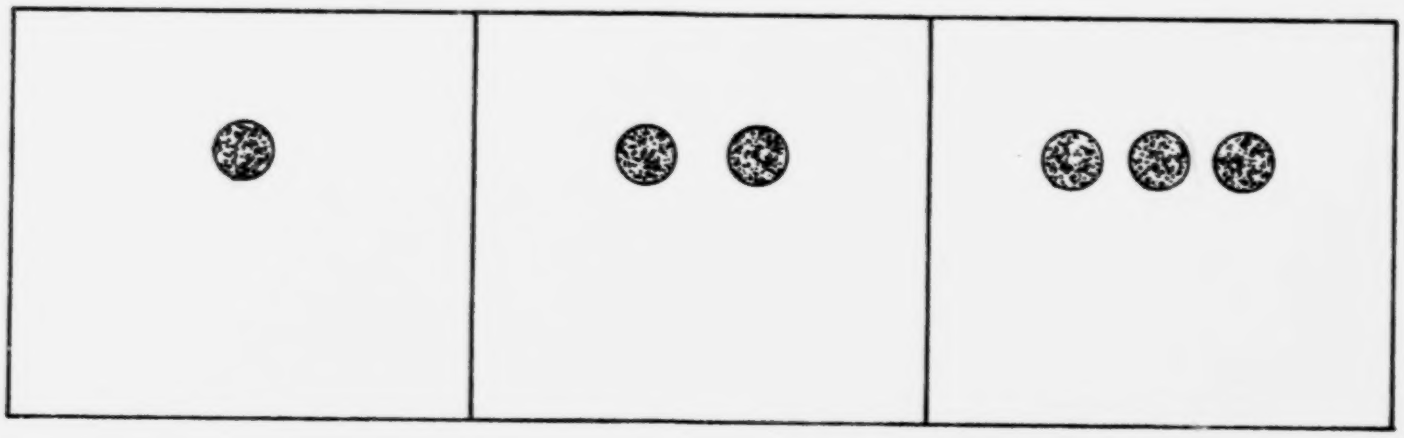
To have concrete models effectively aid in learning, they need to be used consistently enough so that children can grasp the mathematical principles exemplified. Using a unique model, new to learners for a short period of time does not result in any better learning than does use of a meaningful symbolic model.

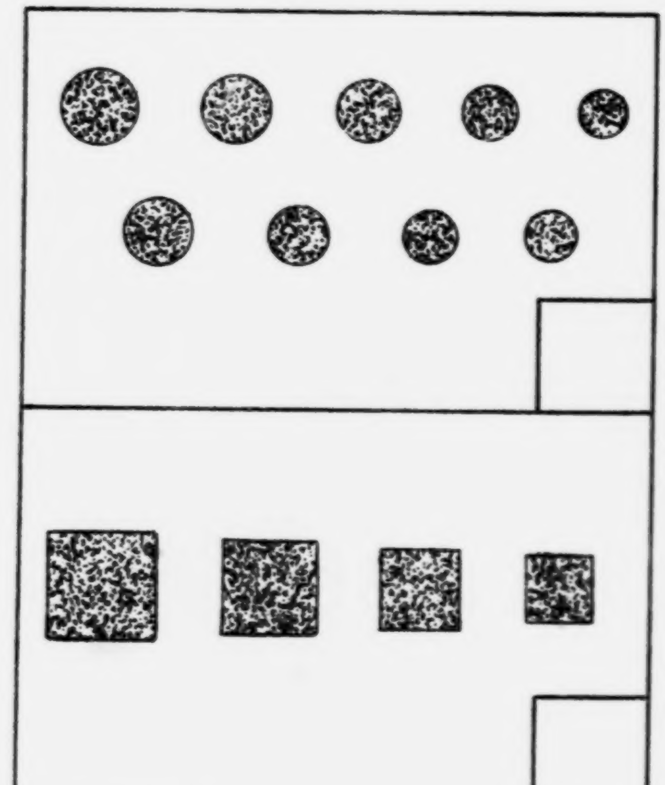
The results of the concrete transfer test raise an interesting question for teaching. Had these children learned mathematics as something useful only in school? Is the present emphasis upon teaching the interrelationships of mathematical principles an over-emphasis which makes no provision for teaching the many diverse applications of mathematical ideas? When mathematical principles were taught meaningfully, that is in relation to other mathematical principles, children were not able to see the application to other concrete models. If part of the objectives of mathematics education is to enable children to use mathematical ideas in a variety of ways, then perhaps the mathematics programs need to be expanded to include provision for teaching the applications. Merely teaching mathematics as a structure of interrelated ideas does not ensure that children can apply principles outside the area of mathematics.

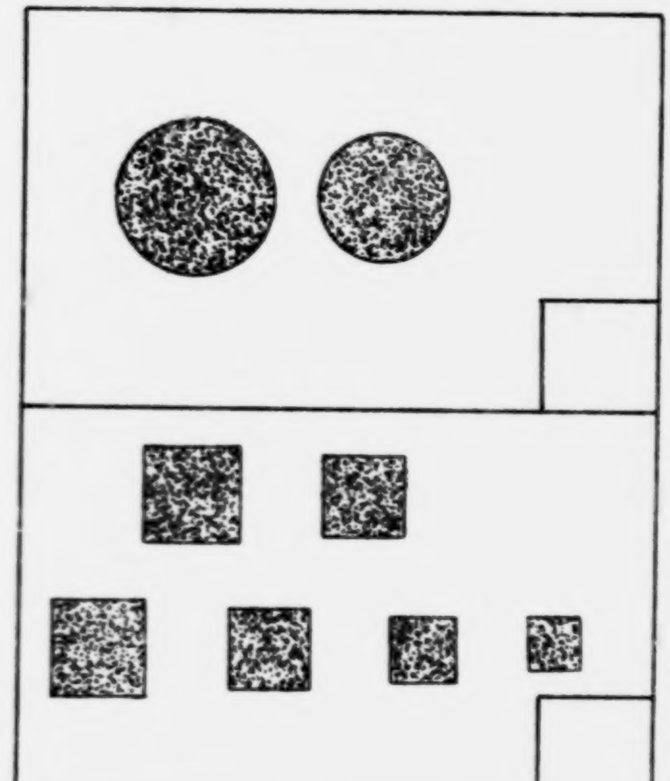
APPENDIX A

QUALIFYING EXAMINATION AND TESTS OF DEPENDENT VARIABLES

QUALIFYING EXAMINATION







3	6	3	4	7
+3	+6	+4	+7	+5

7	9	8	1	3
+9	+4	+9	+6	+8

2	2	4	4	3
+8	+8	+4	+8	+6

5	1	3	6	8
+3	+9	+7	+3	+8

RECALL TEST

Name ______________ Group ______

4, 2 → ______

10, ______ → 10

______, 4 → 8

______, 1 → 5

4, ______ → 4

______, 6 → 6

3 2 →

1, 5 → ______

9, ______ → 9

______, 1 → 6

1, 4 → ______

1, ______ → 8

______, 3 → 3

____, 9 → ____

1, 10 → ____

3, ____ → 9

2, ____ → 10

____, 1 → 8

5, 2 → ____

2, ____ → 4

____, 1 → 2

4, ____ → 4

1, 7 → ____

2, ____ → 6

____, 1 → 7

1, 2 → ____

3, ____ → 3

SYMBOLIC TRANSFER

TEST I AND II

Name ______ Group ___

3, 4 → ____

____, 1 → 11

13, ____ → 13

3, 5 → ____

____, 1 → 16

2, ____ → 12

____, 2 → 16

2, ____ → 16

5, 3 → ____

____, 1 → 15

1, ____ → 13

1, 15 → ____

Name ____________ Group ____

4, ____ → 12

1, 11 → ____

____, 2 → 12

12, ____ → 12

14, 1 → ____

____, 12 → 12

1, ____ → 16

4, 4 → ____

____, 2 → 14

2, ____ → 14

1, 14 → ____

CONCRETE TRANSFER TEST

INSTRUCTIONS AND DATA SHEETS

TEST C

I. Pre-Test Procedure

Subject and tester should be seated at a table with the balance beam between them. The non-numeral side of the balance beam should be facing the subject.

A. "Do you know what balancing is? Look at the point of the bar. When it points straight up and the bar is level with the table, the sides are balanced. Let's practice and see if you can tell me when it is balanced."

Place one wt. on 5 and one on 5. "Is it balanced?"

Place one wt. on 10 and one on 8. "Is it balanced?"

Place one wt. on 7 and one on 8. "Is it balanced?"

Place one wt. on 2 and two on 1. "Is it balanced?"

As soon as the child is answering without hesitation go on to the next part.

B. Turn the beam around so subject can see numbers.

"Put your finger on one. Where is two? Show me where 3 is," etc. with 4-10. (Object is to make sure subject knows which direction numbers go.)

II. C-I Test START STOP WATCH

"Here is a weight for you." Hand the subject one weight. "I am going to put some weights on this side of the bar and I want you to make the bar balance by putting your weight on your side." For each set say: "I am putting "4" weights on '2'." Follow the sequence of weights and locations on record sheet.

Record the number of trials needed to balance.

STOP WATCH Record time.

III. C-II Test START STOP WATCH

"Now I am going to put some weights on my side and I want you to balance it with the number of weights I give you."

For each set of weights say: "I will put one on '4' and you balance it with '2' weights."

Record the number of trials needed to balance each set.

STOP WATCH Record time.

Record Sheet

Subject ______________________ Tester ______________________

C-I

Directions
START WATCH

Number of Weights	Location	Balance	Number of Trials
4	2	8	
1	9	9	
10	1	10	
1	10	10	
2	4	8	
3	3	9	
5	1	5	
2	5	10	
1	5	5	
8	1	8	
9	1	9	
5	2	10	
6	1	6	

STOP WATCH Time ________ Total Trials

Record Sheet

Subject ______________________ Tester ______________________

C-II

Directions
START WATCH

Place one weight on	Hand Subject	Balance	Number of Trials
4	2	2	
4	4	1	
2	2	1	
7	7	1	
6	6	1	
6	3	2	
6	2	3	
7	1	7	
8	8	1	
2	1	2	
3	3	1	
3	1	3	

STOP WATCH Time __________ Total Trials

APPENDIX B

QUALIFYING EXAMINATION AND INTELLIGENCE TEST DATA AND ANALYSES

TABLE 1

MEAN SCORES AND STANDARD DEVIATIONS FOR TREATMENT GROUPS ON KUHLMAN-FINCH INTELLIGENCE TEST II

Treatment								
I			II			III		
Group	Mean	SD	Group	Mean	SD	Group	Mean	SD
D	112.36	14.78	A	120.27	8.68	B	107.11	13.20
E	111.58	15.92	F	108.36	5.73	C	115.50	10.85
G	112.58	10.43	J	109.85	10.84	H	111.27	15.18
L	112.15	7.13	K	111.33	13.65	I	111.46	12.39

TABLE 2

ANALYSIS OF VARIANCE OF TREATMENT GROUP MEANS ON KUHLMAN-FINCH INTELLIGENCE TEST II

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	2.70	2	1.35	.100	.906
Within	121.66	9	13.52		

TABLE 3

ANALYSIS OF VARIANCE OF TREATMENT GROUP MEANS ON QUALIFYING EXAMINATION

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	.42	2	.21	.583	.578
Within	3.21	9	.36		

TABLE 4

ANALYSIS OF VARIANCE OF SCORES OF INDIVIDUALS WITHIN TREATMENTS ON THE KUHLMAN-FINCH INTELLIGENCE TEST II

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	64.34	2	32.17	.23	.79
Within	18302.01	131	139.71		

TABLE 5

ANALYSIS OF VARIANCE OF SCORES OF INDIVIDUALS WITHIN TREATMENTS ON THE QUALIFYING EXAMINATION

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	7.54	2	3.77	.95	.39
Within	521.38	131	3.98		

TABLE 6

ANALYSIS OF VARIANCE OF EXPERIMENT GROUPS MEANS ON QUALIFYING EXAMINATION

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	.002	1	.002	.008	.93
Within	1.50	6	.250		

TABLE 7

ANALYSIS OF VARIANCE OF EXPERIMENTAL GROUPS MEANS ON KUHLMAN-FINCH INTELLIGENCE TEST II

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	.16	1	.16	.011	.919
Within	86.45	6	14.41		

APPENDIX C

TEACHER OBSERVATION SHEET

TEACHER OBSERVATION SHEET

DATE ____________ OBSERVER ____________ GROUP OBSERVED ____________

Pupil			Teacher		
Concrete	Semi-Concrete	Symbolic	Concrete	Semi-Concrete	Symbolic

	Pupil	Teacher
Total Tabulations		
% Concrete		
% Semi-Concrete		
% Symbolic		

DATE ____________ OBSERVER ____________ GROUP OBSERVED ____________

Concrete	Semi-Concrete	Symbolic	Concrete	Semi-Concrete	Symbolic

	Pupil	Teacher
Total Tabulations		
% Concrete		
% Semi-Concrete		
% Symbolic		

APPENDIX D

STUDENT WORK SHEETS

Worksheet I

Name ______________________ **Group** ______________________

2 + 2 + 2 + 2 + 2 = ☐

5,2 → ☐

5 + 5 = ☐

2,5 → ☐

3 + 3 + 3 = ☐

3,3 → ☐

9 = ☐

1,9 → ☐

1 + 1 + 1 + 1 + 1 + 1 + 1
1 + 1 + 1 = ☐

10,1 → ☐

10 = ☐

1,10 → ☐

1 + 1 + 1 + 1 + 1 + 1 + 1
1 + 1 = ☐

9,1 → ☐

2,5 → ☐

5 + 5 = ☐

Worksheet II

Name ______________________ Group ______________________

10 → 5, _____ 2 + 2 + 2 + 2 + 2 = _____	10 → 10, _____ 1 + 1 + 1 + 1 + 1 + 1 + 1 + 1 + 1 + 1 = ______
10 → 2, _____ 5 + 5 = ______	9 → 3, _____ 3 + 3 + 3 = ______
9 → 9, _____ 1 + 1 + 1 + 1 + 1 + 1 + 1 + 1 + 1 = ______	10 → 10 10 = _____
_____ → 5,2 2 + 2 + 2 + 2 + 2 = ______	_____ → 3,3 3 + 3 + 3 = _______

Worksheet III

Name ______________________ Group ______________________

5,2 → _____	3,3 → _____
2,5 → _____	9,1 → _____
1,10 → _____	1,9 → _____
10,1 → _____	2,5 → _____

Name ______________________ Group ______________________

10 → 5, _____

10 → 10, _____

10 → 2, _____

9 → 3, _____

9 → 9, _____

_____ → 5,2

_____ → 3,3

Worksheet V

Name ________________________ **Group** ____________________________

_____, _____ → _____

_____, _____ → _____

_____, _____ → _____

_____, _____ → _____

_____, _____ → _____

_____, _____ → _____

_____, _____ → _____

_____, _____ → _____

Worksheet VI

Name ____________________ Group ____________________________

5,2 → ____	1 + 1 + 1 + 1 + 1 + 1 + 1 + 1 + 1 + 1 = ____
1, ____ → 10	3 + 3 + 3 = ____
____ → 2,5	5 + 5 = ____
10,1 → ____	4 + 4 = ____
____, 3 → 9	2 + 2 + 2 + 2 + 2 = ____
9, ____ → 9	2 + 2 + 2 + 2 = ____
2,4 → ____	10 = ____
____, 8 → 8	8 = ____
8 → 4, ____	1 + 1 + 1 + 1 + 1 + 1 + 1 + 1 + 1 = ____
____ → 1,8	8 = ____

Worksheet VIIa

Name ______________________________ Group ___________________________

5,2 → _____

1, _____ → 10

_____ → 2,5

_____,3 → 9

9, _____ → 9

Worksheet VIIb

Name ______________________ Group ______________________

2,4 → _____

_____, 8 → 8

8 → 4, _____

_____ → 1,8

1,9 → _____

Worksheet IX

Name ______________________ Group ______________________

10 → 1, ____	____ → 9,1
8 → ____ ,2	6 → 3, ____
____ → 1,6	8 → ____ ,4
4 → ____ ,4	____ , 3 → 3

Worksheet X

Name ______________________ Group ______________________

_____ → 5,2	6 → _____,3
_____ → 3,3	5,1 → _____
8 → 2, _____	2, _____ → 4
7 → 7, _____	_____, 3 → 3

Name _______________ Group _______________

10 → 2, _____	_____ → 1,9
8 → _____,8	_____ → 1,7
2,_____ → 6	1,5 → _____
4,1 → _____	_____, 1 → 3

Worksheet XII

Name ______________________ Group ______________________

2,5 → _____

_____ ,3 → 9

2, _____ → 8

2,3 → _____

4, _____ → 4

_____ ,2 → 1

10 → _____ ,5

9,1 → _____

8 → 2, _____

6 → _____ ,2

_____ ,4 → 8

_____ ,3 → 6

_____ → 4,2

10 → _____ ,2

1, _____ → 8

7 → 7,1

_____ → 3,3

_____ → 2,3

4 → 2, _____

_____ ,2 → 10

Worksheet XIV

Name ____________________ Group ________________

2,5 → 9	1,7 → 7
4,2 → 8	3,2 → 5
7,1 → 1	3,3 → 3
3,3 → 9	2,5 → 8
6,1 → 6	1,8 → 8
10 → 5,2	7 → 7,1
4,4 → 4	6 → 1,6
2,4 → 8	4 → 2,3
5 → 5,2	10 → 3,3
3,3 → 9	9 → 1,9

Worksheet XV

Name _______________ Group _______________

10 → _____ ,5

8 → 2, _____

2,_____ → 4

3,3 → _____

6 → 2,_____

2,_____ → 10

3,2 → _____

10 → 2, _____

9 → _____ ,3

5,2 → _____

4 → 2, _____

5,2 → _____

9 → _____ ,3

2,4 → _____

4,_____ → 8

5, _____ → 10

_____ ,3 → 6

2,3 → _____

8 → _____ ,4

6 → 3,_____

Worksheet XVI

Name ____________________ Group ____________________

9 → 1,9	5 → 5,2
10 → 3,3	2,4 → 8
2,3 → 4	4 → 4,2
6 → 1,6	5,2 → 10
8 → 5,2	6 → 6,1
3,3 → 9	2,2 → 2
7 → 7,1	2,3 → 4
3,3 → 6	8 → 4,2
5 → 3,2	5,2 → 7
1,6 → 6	2,2 → 10

APPENDIX E

DAILY INFORMATION SHEET

Date ____________

DAILY INFORMATION SHEET

	Group A	Group B	Group C
Objective			
Activity			
Materials Used			
Length of Period			
Interruptions			
Number Absent			

Group D	Group E	Group F	Group G	Group H

APPENDIX F

ANALYSES OF VARIANCE OF THE SCORES BY TESTER ON CONCRETE TRANSFER TEST: TIME AND NUMBER OF TRIALS, PART I AND PART II

TABLE 1

ANALYSIS OF VARIANCE OF THE SCORES BY TESTER ON CONCRETE TRANSFER TEST: TIME, PART I

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	68941.0	5	13788.2	1.55	.1790
Within	1138519.0	128	8894.7		

TABLE 2

ANALYSIS OF VARIANCE OF THE SCORES BY TESTER ON CONCRETE TRANSFER TEST: TIME, PART II

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	37319.2	5	7463.8	.82	.54
Within	1169098.2	128	9133.6		

TABLE 3

ANALYSIS OF VARIANCE OF SCORES BY TESTERS ON CONCRETE TRANSFER TEST: NUMBER OF TRIALS, PART I

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	384.9	5	77.0	.56	.73
Within	17734.4	128	138.5		

TABLE 4

ANALYSIS OF VARIANCE OF SCORES BY TESTERS ON CONCRETE TRANSFER TEST: NUMBER OF TRIALS, PART II

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	283.5	5	56.7	.32	.90
Within	22928.6	128	179.1		

APPENDIX G

CONCRETE TRANSFER TEST: DATA AND ANALYSES PART I AND PART II

TABLE 1

MEAN SCORES AND STANDARD DEVIATIONS OF GROUPS ON CONCRETE TRANSFER TEST: NUMBER OF TRIALS, PART I

Treatment I			Treatment II			Treatment III		
Group	Mean	SD	Group	Mean	SD	Group	Mean	SD
D	36.5	9.49	A	36.8	20.30	B	29.7	12.18
E	33.7	7.10	F	33.9	8.03	C	36.8	11.82
G	38.2	10.12	J	34.4	9.69	H	41.4	14.39
L	36.0	12.12	K	33.1	10.95	I	31.6	9.55
Treatment Mean	36.1		Treatment Mean	34.5		Treatment Mean	34.9	
			Grand Mean	35.2				

TABLE 2

MEAN SCORES AND STANDARD DEVIATIONS OF GROUPS ON CONCRETE TRANSFER TEST: NUMBER OF TRIALS, PART II

Treatment I			Treatment II			Treatment III		
Group	Mean	SD	Group	Mean	SD	Group	Mean	SD
D	32.3	12.81	A	40.6	19.51	B	35.2	23.67
E	31.8	12.04	F	31.4	12.18	C	35.6	15.91
G	36.7	12.26	J	35.6	9.45	H	36.0	13.75
L	32.7	8.53	K	38.5	16.27	I	32.2	11.60
Treatment Mean	33.4		Treatment Mean	36.5		Treatment Mean	34.8	
			Grand Mean	34.9				

TABLE 3

MEAN SCORES AND STANDARD DEVIATIONS OF GROUPS ON CONCRETE TRANSFER TEST: TIME, PART I

Treatment I			Treatment II			Treatment III		
Group	Mean	SD	Group	Mean	SD	Group	Mean	SD
D	376.1	61.94	A	384.4	152.79	B	368.2	99.52
E	349.9	60.35	F	355.6	129.28	C	406.5	96.77
G	424.3	126.90	J	381.6	85.54	H	458.1	123.11
L	382.5	79.08	K	393.8	65.88	I	346.1	44.90
Treatment Mean		383.2	Treatment Mean		378.9	Treatment Mean		394.7
			Grand Mean	385.6				

TABLE 4

MEAN SCORES AND STANDARD DEVIATIONS OF GROUPS ON CONCRETE TRANSFER TEST: TIME, PART II

Treatment I			Treatment II			Treatment III		
Group	Mean	SD	Group	Mean	SD	Group	Mean	SD
D	335.2	80.18	A	388.4	135.31	B	359.9	157.70
E	348.9	63.49	F	330.7	132.64	C	379.8	91.92
G	365.1	103.19	J	385.8	99.23	H	387.3	105.46
L	349.1	88.66	K	404.8	89.51	I	341.0	92.92
Treatment Mean		349.6	Treatment Mean		377.4	Treatment Mean		367.0
			Grand Mean	364.6				

TABLE 5

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL TREATMENT GROUPS ON CONCRETE TRANSFER TEST: TIME, PART I

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	37.80	1	37.80	.062	.812
Within	3654.68	6	609.11		

TABLE 6

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL TREATMENT GROUPS ON CONCRETE TRANSFER TEST: TIME, PART II

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	1551.24	1	1551.24	2.606	.158
Within	3571.76	6	595.29		

TABLE 7

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL TREATMENT GROUPS ON CONCRETE TRANSFER TEST: NUMBER OF TRIALS, PART I

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	4.91	1	4.91	1.659	.245
Within	17.77	6	2.96		

TABLE 8

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL TREATMENT GROUPS ON CONCRETE TRANSFER TEST: NUMBER OF TRIALS, PART II

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	20.03	1	20.03	1.90	.217
Within	63.26	6	10.54		

TABLE 9

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL AND CONTROL GROUPS ON CONCRETE TRANSFER FEST: TIME, PART I

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	536.57	2	268.29	.222	.805
Within	10877.30	9	1208.59		

TABLE 10

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL AND CONTROL GROUPS ON CONCRETE TRANSFER TEST: TIME, PART II

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	1583.91	2	791.96	1.463	.282
Within	4873.03	9	541.45		

TABLE 11

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL AND CONTROL GROUPS ON CONCRETE TRANSFER TEST: NUMBER OF TRIALS, PART I

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	5.44	2	2.72	.239	.792
Within	102.52	9	11.39		

TABLE 12

ANALYSIS OF VARIANCE OF MEAN SCORES OF EXPERIMENTAL AND CONTROL GROUPS ON CONCRETE TRANSFER TEST: NUMBER OF TRIALS, PART II

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	20.12	2	10.06	1.256	.330
Within	72.12	9	8.01		

APPENDIX H

ANALYSES OF VARIANCE OF INDIVIDUAL SCORES WITHIN TREATMENT GROUPS FOR ALL MEASURES OF DEPENDENT VARIABLES

TABLE 1

ANALYSIS OF VARIANCE OF INDIVIDUAL SCORES ON RECALL TEST (EXPERIMENTAL GROUPS)

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	89.54	1	89.54	5.61	.020*
Within	1450.54	91	15.95		

TABLE 2

ANALYSIS OF VARIANCE OF INDIVIDUAL SCORES ON SYMBOLIC TRANSFER TEST I (EXPERIMENTAL GROUPS)

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	282.00	1	282.00	9.07	.003**
Within	2827.37	91	31.07		

TABLE 3

ANALYSIS OF VARIANCE OF INDIVIDUAL SCORES ON SYMBOLIC TRANSFER TEST II (EXPERIMENTAL GROUPS)

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	671.54	1	671.54	24.51	.0001**
Within	2493.40	91	27.40		

TABLE 4

ANALYSIS OF VARIANCE OF INDIVIDUAL SCORES ON CONCRETE TRANSFER TEST, PART I TIME: ALL GROUPS

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	2512.26	2	1256.13	.137	.872
Within	1204947.17	131	9198.07		

TABLE 5

ANALYSIS OF VARIANCE OF INDIVIDUAL SCORES ON CONCRETE TRANSFER TEST, PART II TIME: ALL GROUPS

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	20681.58	2	10340.79	1.14	.322
Within	1185734.71	131	9051.41		

TABLE 6

ANALYSIS OF VARIANCE OF INDIVIDUAL SCORES ON CONCRETE TRANSFER TEST, TOTAL TIME: ALL GROUPS

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	12733.48	2	6366.74	.23	.797
Within	3663952.10	131	27969.10		

TABLE 7

ANALYSIS OF VARIANCE OF INDIVIDUAL SCORES ON CONCRETE TRANSFER TEST, NUMBER OF TRIALS, PART I: ALL GROUPS

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	98.00	2	49.00	.36	.701
Within	18021.67	131	137.57		

TABLE 8

ANALYSIS OF VARIANCE OF INDIVIDUAL SCORES ON CONCRETE TRANSFER TEST, NUMBER OF TRIALS, PART II: ALL GROUPS

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	310.90	2	155.45	.89	.413
Within	22901.42	131	174.82		

TABLE 9

ANALYSIS OF VARIANCE OF INDIVIDUAL SCORES ON CONCRETE TRANSFER TEST, TOTAL NUMBER OF TRIALS: ALL GROUPS

Source of Variation	Sum of Squares	df	Mean Square	F ratio	p
Between	232.40	2	116.20	.265	.767
Within	57243.07	131	436.97		

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